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THE SIGNIFICANCE OF THE AUDITORY ELECTRICAL EFFECTS FOR AUDITORY THEORY

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In a recent paper,[1] a survey was made of the various investigations concerned with the electrical effects produced in the neighborhood of the auditory apparatus by sound stimuli. Its purpose was to determine whether any of the investigations provided conclusive evidence that the electrical effects reported were due to auditory action currents, and to determine the extent to which the conditions and problems of the various investigations were comparable. The aim of the present paper is to discuss the significance which these electrical effects may have for auditory theory, particularly in the light of new evidence that has appeared in recent papers.

The theories upon which the auditory electrical effects might be expected to have the most direct bearing are the frequency theories. The term "frequency theory" is used here to refer to any of the theories which assume that the entire range of audible frequencies may be retained in the response of the nerve. It was supposed at first that this type of hypothesis required the assumption for the fibers of the auditory nerve of a refractory period considerably less than that known to occur in other mammalian nerves. Thus, a given fiber, to be capable of responding at frequencies as high as 20,000 a second, must have an absolute refractory period not greater than 1/20,000 of a sec. But from measurements in other nerves, it did not seem likely that any fiber could respond steadily at frequencies much greater than 1000 a sec.[2] Troland,[3] however, was able to avoid this diffi-

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[1] G. Kreezer, A critical examination of the investigations of auditory action currents, this JOURNAL, 44, 1932, 638-676.

[2] Cf. H. Rosenberg and K. Kitayama, Untersuchungen über Nervenaktionsströme, *Arch. f. d. ges. Physiol.*, 223, 1930, 723.

[3] L. T. Troland, The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 43 f.

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culty by suggesting a type of theory which has been designated by some writers as the volley theory. Troland pointed out that even though no fiber of the auditory nerve responded at frequencies greater than 1000 per sec., the nerve as a whole might show frequencies much greater if it were assumed that *different* fibers discharged in response to immediately successive periods of the stimulating wave. A basis for such behavior would be provided, according to him, if differences in excitability occurred among the fibers terminating in the region of the basilar membrane under vibration. Thus, granted that the basilar membrane vibrates at the frequency of the stimulus, the following behavior might be expected for the frequencies above 1000. Corresponding to the first cycle of the wave, only the hair cells of maximum excitability would be set off; but they would not be able to respond at the next cycle on account of still being in the absolute refractory state. The cells of next higher threshold, which had not been set off at the first cycle, might, however, respond at the second cycle, because of the summation in them of the excitation process due to both the first and second cycles of the stimulating wave. Thus the process might continue until the fibers that have responded first have recovered from their refractory condition and are able to respond again. Troland's hypothesis thus supplied a way to avoid the difficulties raised by the question of the absolute refractory period. This type of theory, although it made the frequency doctrine more plausible, did not, of course, show that it was true.

The term "volley theory" may be somewhat misleading as a name for this type of hypothesis. In investigations of motor nerves, the term "volley" has been used to designate an aggregation of impulses fired from a group of fibers in response to a single stimulus.[4] The term could be applied to each of a series of such aggregates or groups of impulses whether the successive aggregates were fired by the same set of fibers or different sets. The distinctive feature of Troland's and other volley theories, on the other hand, is not so much that volleys occur in the nerve response, but that the immediately successive volleys fired by the nerve at the higher frequencies are provided by *different* sets of fibers. The volleys from different sets of fibers are thus staggered in time so that a new set discharges for each immediately successive period of the stimulating wave, up to a certain point.[5] Such staggering is necessary if, as the volley theories assume, the nerve as a whole is to transmit the entire range of audible frequencies without exceeding the frequency limits considered probable in the individual fibers.

[4] R. S. Creed, D. Denny-Brown, J. C. Eccles, E. G. T. Liddell and C. S. Sherrington, *Reflex Activity of the Spinal Cord*, 1932, 3.

[5] Troland, as we have seen, assumes differences in the excitability of different fibers to account for the staggering feature of his volley theory. Wever and Bray suggest, instead, differences in refractory period of the fibers to account for the staggering. (E. G. Wever and C. W. Bray, Present possibilities for auditory theory, *Psychol. Rev.*, 37, 1930, 376 f.)

It would seem that the question of whether the higher audible frequencies can be transmitted by the auditory nerve should be capable of direct answer by experiments designed to detect the action potentials which occur in this nerve in response to sound waves of various frequencies. The earlier attempts for detecting the action potentials of the eighth nerve were made, however, with instruments incapable of responding at the higher frequencies.[6] It occurred to Wever and Bray to repeat this type of experiment, but with an amplifier and telephone as the indicator of the characteristics of any electrical effects that might be produced instead of the more slowly responding string galvanometer.[7] When they did so, they found that the electrical currents picked up by an electrode on the eighth nerve provided telephone tones of the pitch of the stimuli up to frequencies as high as 5200 cycles. The checks they performed to rule out the possibility of artifacts satisfied them that the effect was due to action currents in the auditory nerve. Their results were therefore interpreted as lending support to a frequency type of auditory theory.[8] Experiments of a similar sort were subsequently performed by a number of other investigators. The results of these investigations have been reviewed in the paper of which mention has already been made, and the conclusion drawn that none of the investigations considered provided conclusive evidence that the electrical effects reported were due to auditory action currents.[9]

Before proceeding now to a consideration of the present status of the question, it is necessary to consider the properties of the different electrical effects so far reported and to specify the terms to be used in designating them. Davis and Saul,[10] in recent papers, have emphasized the fact that two different physiological effects may be produced upon stimulation of the ear by sound, and have summarized the properties of these two effects as determined in their investigations. The one effect they refer to as "true action currents," the other as electrical or "cochlear spread." The properties with respect to which the two effects are described are (1) the range of frequencies of the effect as a function of the frequency of the stimu-

[6] F. J. Buytendijk, On the negative variation of the nervus acusticus caused by a sound, *K. Akad-Wetensch., Amsterdam, Proc. Sci. Sect.*, Eng. trans., 13, 1911, 649-652; A. Forbes, R. H. Miller, and J. O'Connor, Electrical responses to acoustic stimuli in the decerebrate animal, *Amer. J. Physiol.*, 80, 1927, 363-380.

[7] E. G. Wever and C. W. Bray, The nature of acoustic response, *J. Exper. Psychol.*, 13, 1930, 373-387.

[8] Wever and Bray, Present possibilities for auditory theory, *Psychol. Rev.*, 37, 1930, 374-380.

[9] G. Kreezer, *op. cit.*, 674-676.

[10] L. J. Saul and H. Davis, Electrical phenomena of the auditory mechanism, *Trans. Amer. Otol. Soc.*, 22, 1932, 137-142; Action currents in the central nervous system, I. Action currents of the auditory tracts, *Arch. Neurol. and Psychiat.*, 28, 1932, 1104-1116; Auditory action currents, this JOURNAL, 45, 1933, 358-359.

lus, (2) the form characteristics of the effect, (3) the velocity of propagation, or the latency between the arrival of sound waves in the cochlea and the appearance of the electrical effect in some particular tissue, (4) the place of occurrence of the effect, (5) the influence of general and local narcotics, and (6) the influence of death.

In terms of these properties, "cochlear spread" is designated by Davis and Saul:[11] (1) as reproducing the frequency of the stimulating sound up to at least 5000 cycles; (2) as reproducing the wave form of the stimulus above 800 per sec., although at lower pitches the response may be more complex (observations made by means of the cathode ray oscillograph); (3) as not restricted to the auditory tracts but broadcasting throughout all the tissues of the head; (4) as showing a latency in its appearance anywhere in the head, after the arrival of sound waves in the cochlea, of less than 0.1 σ; (5) as being very resistant to narcosis; and (6) as persisting (although with reduced intensity) many minutes after death.

The descriptions which the other investigators have given of the effects reported by them show different degrees of completeness with respect to the various properties so systematically examined by Davis and Saul. Nevertheless, when we examine the properties which *are* reported by the other investigators, we find that they are like those specified by Davis and Saul for cochlear spread. Thus, with respect to frequency, Wever and Bray[12] report a correspondence in frequency of stimulus and electrical effect up to frequencies of about 5000 (5200) cycles, and state that speech can be readily transmitted. Adrian,[13] Hughson and Crowe,[14] and Rademaker and Bergansius[15] report that they confirm this finding. Wever and Bray, and Adrian report that the effect is resistant to narcotics injected into the nerve tissues,[16] and that it persists for some time after the death of the animal.[17] Wever and Bray, Adrian, and Rademaker and Bergansius all indicate that the effect detected reproduces the wave form of the stimulus.[18]

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[11] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 358 f.

[12] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 377.

[13] E. D. Adrian, The microphonic action of the cochlea: an interpretation of Wever and Bray's experiments, *J. Physiol.*, 71, 1931, xxviii-xxix.

[14] W. Hughson and C. J. Crowe, Function of the round window, *J. Amer. Med. Assoc.*, 96, 1931, 2027-2028.

[15] G. G. I. Rademaker and F. L. Bergansius, Expériences sur la physiologie de l'ouie, *Arch. neerl. de physiol.*, 16, 1931, 346-349.

[16] Adrian, *op. cit.*, xxix; Wever and Bray, Action currents in the auditory nerve in response to acoustic stimulation, *Proc. Nat. Acad. Sciences*, 16, 1930, 248.

[17] E. D. Adrian, D. W. Bronk, and G. Phillips, The nervous origin of the Wever and Bray effect, *J. Physiol.*, 73, 1931, 2P-3P; Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 382.

[18] Cf. Kreezer, *op. cit.*, 673.

Wever and Bray, and Adrian report that it can be detected in tissues remote from the auditory tracts.[19]

Thus the properties listed by Davis and Saul as characteristic of cochlear spread are substantially like those which have been given as characteristic of the electrical effects reported in the other investigations.[20] Since this effect has come to be known as the "Wever-Bray effect," we shall refer to it by that name instead of as "cochlear spread." This does not imply, however, that it is valid to assume that the effect is due to identical phenomena in the different experiments.[21]

The other effect reported by Davis and Saul is designated by them as "true action currents." The specifications given concerning its properties are in direct contrast with those given for "spread." The true action currents of Davis and Saul are described:[22] (1) as following the frequency of the stimulating waves up to but not over 900 or 1000 per sec.; (2) as appearing on the cathode ray oscillograph as waves of the shapes and time relations characteristic of action currents in peripheral nerves; (3) as being localized in the auditory tracts, and as being more intense in the larger tracts and centers than in the eighth nerve; (4) as traveling with the usual speeds of nerve impulses; (5) as showing the usual sensitivity to local or general anaesthetics; and (6) as disappearing just before or instantly at death. An examination of the reports of the other investigators fails to reveal in any of them an effect with properties like that reported by Davis and Saul for true action currents. Davis and Saul are apparently the only investigators who have isolated this particular effect. We shall therefore refer to it as the "Davis-Saul effect."

Theoretical Significance of the Electrical Effects

We may now consider the significance which these effects may have for certain questions of importance for auditory theory.

Davis-Saul effect: (a) Cause. Certain of the lines of evidence given by Davis and Saul to prove that the Davis-Saul effect is due to action currents were considered in our previous review.[23] The two new lines of evi-

[19] Adrian, *op. cit.*, xxviii; Wever and Bray, *op. cit., J. Exper. Psychol.*, 13, 1930, 382.

[20] T. H. Bast, et. al. (The electrical currents associated with sound reception by the ear, *Proc. Soc. Exper. Biol. & Med.*, 30, 1933, 638-640) have recently reported observations on the Wever-Bray effect in the dog. The effect here too has properties like those summarized above with respect to frequency, place of occurrence, resistance to narcotics, and persistence after death.

[21] Cf. Kreezer, *op. cit.*, 648 f.

[22] Saul and Davis, *op. cit.*, this Journal, 45, 1933, 359.

[23] Kreezer, *op. cit.*, 664-668.

dence presented in their more recent papers relate to the form characteristics of the effect and to its velocity of propagation.

The discoverers of the Davis-Saul effect report (1) that the effect appears "on the cathode ray oscillograph as waves of shapes and time relations characteristic of action currents in peripheral nerves," and (2) that it travels "with the usual speed of nerve impulses."[24] These findings undoubtedly lend strong support to their conclusion that this effect is due to action currents in the auditory nerve and tracts. Assuming that this conclusion is correct, let us now examine its consequences for the different types of auditory theory, and consider some of the questions which may be asked concerning the detailed nature of the events in the nerve.[25]

(b) Bearing on frequency theories. Frequency theories, it will be recalled, are of two types: (1) the 'telephone theories,' which assume that the entire range of auditory frequencies are transmitted by the individual fibers; and (2) the 'volley theories,' which assume that the higher frequencies appear only in the response of the nerve as a whole, and that the individual fibers are transmitting impulses at lower frequencies.

The report of Davis and Saul that the frequency correspondence of stimulus and total nerve response does not go beyond 1000 cycles per sec., has bearing strictly only on those frequency theories which are concerned with the response in the nerve as a whole. The results of Davis and Saul demonstrate the incorrectness of the hypothesis that the frequency of response of the nerve as a whole can extend throughout the whole range of audible frequencies. The volley theories, which were designed to account for the transmission of the higher frequencies by the nerve as a whole, are thus found to disagree with the facts.

The determination of the frequency limitation for the whole nerve does not, however, indicate decisively whether or not a correspondence obtains between the frequency of the stimulus and the response of the *individual fibers* of the nerve, and through what range this correspondence holds, if it occurs at all. The correlation suggested in the telephone type of theory may be improbable on other grounds, but so far as the existent data on the frequencies and wave-forms exhibited by the Davis-Saul effect goes, it still remains a possibility.

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[24] Saul and Davis, *op. cit.*, this JOURNAL, 359.

[25] In the rest of this paper, the successive waves of the Davis-Saul effect which correspond to the successive cycles of the stimulating waves will be referred to as volleys. For these waves were detected in the nerve as a whole, and so, presumably, are due to the combined effect of action potentials from many coöperating fibers. In our reference to these waves as volleys, no implication is being made concerning the presence or absence of the stagger arrangement specified in the *volley theories*. Cf. page 2, above.

(c) The activity of the individual fibers. An essential step in the determination of the correlations that exist between the attributes of auditory experience and events in the auditory nerve is, naturally, the preliminary determination of the precise nature of the events in the nerve. As we have seen, the data so far reported on the Davis-Saul effect provide a direct answer to the question of the way the frequency of the volleys of the nerve as a whole depends on the frequency of the stimulus. These data do not, however, permit as yet an answer to certain questions concerning the activity and characteristics of the individual fibers of the nerve. This limitation rests on the fact that the record of a composite curve produced by a number of coöperating fibers does not necessarily permit conclusions to be made concerning the part played by the individual fibers.

Some of the factors, for example, on which the characteristics of the curve representing a single volley may depend are: (1) the type of fibers discharging (the form and duration[28] of the action potential may show differences, as does the velocity of propagation, in different kinds of fibers); (2) the number of fibers discharging (variations in this factor might cause variations in the height and form of the composite wave); (3) the temporal order in which the different participating fibers discharge (variations in this factor might cause variations in both the duration and form of the composite wave). It is evident that without special analysis, based on supplementary data, the wave form representing a particular volley might be interpreted in a number of different ways with respect to the part played by the individual fibers.

Let us now examine the character of some of the problems that still remain concerning the properties of the individual fibers of the auditory nerve.

(1) What is the maximum frequency possible in the response of the individual fibers? Is it 1000 impulses a second, less than 1000 or greater than 1000? Any one of these possibilities still exists.

It is not difficult to see that the frequency of response of individual fibers might be either less than or equal to the frequency of the volleys discharged from the nerve as a whole. It may not be quite so apparent that the response-frequency of individual fibers might be higher than the frequency of the volleys. If it were known that the coöperating fibers all fired synchronously, and that therefore the duration of the volleys was not greater than that of the individual impulses, then, it is true, the fibers could not be firing at rates greater than that shown by the volleys of the nerve as a whole. But none of the data so far available on the Davis-Saul effect

[28] Cf. E. A. Blair and J. Erlanger, Comparison of individual axons in the frog, *Proc. Soc. Exper. Biol. & Med.*, 30, 1933, 728-729; also R. S. Lillie, *Protoplasmic and Nervous Action*, 2nd ed., 1932, 325 ff.

indicate whether the volleys from the nerve are synchronous or asynchronous.

Suppose, for example, that the impulses contributing to a volley occurred asynchronously and so as to build up a wave form of the type exhibited by action potentials, and that the duration of the volleys was not less than twice the duration of a single impulse plus the duration of the intervening refractory period of the fibers. In such a case, a given fiber might discharge twice during the interval of time occupied by a given volley, without the betrayal of this fact in the record of the wave-form of the successive volleys. The frequency of discharge of individual fibers might thus be greater than that of the successive volleys from the whole nerve. Some reasons for expecting asynchronous volleys to occur will be discussed below. That frequencies much greater than 1000 can occur in the individual fibers may seem improbable on the basis of the magnitude of the absolute refractory periods known in other nerves. Such behavior is not, however, as yet excluded by Davis and Saul's results. It remains for further research to decide the question.

(2) What is the relation between the frequencies in the individual coöperating fibers and the frequency of the volleys from the nerve as a whole? This question is closely related to the one above, but is not identical with it. Thus, a given fiber might be capable of responding with a *maximum* frequency of 1000 impulses per sec., but might not do so except at maximum intensities of stimulus, if frequency of discharge is a function of intensity of stimulus. In cases where the frequencies in all of the coöperating fibers *are* less than the frequency of the volleys from the whole nerve, then a *stagger* arrangement such as suggested in the volley theories would seem to be necessary.

(3) Upon what characteristic of the stimulating sound wave does the frequency of response in the individual fiber depend? Does it depend on the frequency of the stimulus, as suggested in the telephone theories, or does it depend on the intensity of the stimulus, as found by Adrian in other nerves? Either of these relations seems to be possible on the basis of the existent data.

(4) What type or types of fiber are present in the auditory nerve? Gasser, Erlanger, and their co-workers, it will be recalled, have found marked differences in the velocity of propagation of action potentials in different fibers, and they have shown that different fibers may be classified as of different types on the basis of such differences. Thus, they distinguish between the A, B, and C types, and within the A group, the α, β and γ types.[27]

[27] J. Gasser and H. S. Erlanger, The compound nature of the action current of nerve as disclosed by the cathode ray oscillograph, *Amer. J. Physiol.*, 70, 1924, 624-666; The rôle played by the sizes of the constituent fibers of a nerve trunk in determining the form of its action potential wave, *Amer. J. Physiol.*, 80, 1927, 522-547;

It would be of interest to determine what type or types of fiber occur in the auditory nerve, in terms of this classification. This question should prove answerable by means of procedures like those used by Gasser, Erlanger and Bishop in other nerves. Davis and Saul's specification concerning the velocity of conduction is not yet specific enough to answer this question. These authors state only that the velocity of the action currents is like that found in other nerves;[28] the quantitative data given are in terms of the differences in time of arrival of the Davis-Saul effect at different points in the auditory tracts, without specification of the distances involved.[29]

(5) Asynchronism of the volleys. What temporal relations obtain among the constituent impulses in a volley? Do coöperating fibers fire synchronously (simultaneously), or is there asynchronism of any particular sort? Existent data fail also to throw light on this question. The type of data which might lead to the solution of this problem would consist of oscillographic records of the form and duration of the neural volleys as a function of various properties of the stimuli (both for natural sound stimuli sent into the ear, and for electrical stimuli applied directly to the nerve). But the data available concerning the form and duration of the volleys in the Davis-Saul effect are limited to the report that the volleys appear on the cathode ray oscillograph as waves of the shapes and time relations characteristic of action currents in peripheral nerves.[30] It is possible to conceive of waves of such characteristics being built up either by synchronous or asynchronous impulses in a volley; the specifications given do not help, therefore, in the solution of the problem mentioned above.

In the study of motor nerves, the question of the asynchronism of the impulses in a volley has been found to be of great importance, both in the interpretation of the mechanical records obtained from the muscles excited by the nerve-volleys and in the explanation of certain features of reflex and voluntary movement.[31] It is not unlikely that the temporal order in which the constituent impulses in a volley discharge may be of equal importance in the explanation of certain phenomena of audition.

There is one particular type of asynchronism among the impulses which, it seems to us, is to be expected on the basis of certain views commonly held concerning the mechanism of stimulation in the cochlea. This hypothe-

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The action potential in fibers of slow conduction in spinal roots and somatic nerves, *Amer. J. Physiol.*, 92, 1930, 43-82.

[28] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 359.

[29] Saul and Davis, *op. cit., Trans. Amer. Otol. Soc.*, 22, 1932, 141.

[30] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 359.

[31] See, for example, J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 1926, 265-490.

sis was discussed in a recent paper as a phase of a frequency theory of hearing.[32] But the connection is not a necessary one. The essential features of the argument involved may therefore be outlined in terms that do not limit it to a frequency theory.

Let it be assumed (1) that corresponding to the cycle of displacements represented in a single period of a stimulating sound wave (or in a fraction of the period, such as one-half a period), there occurs an equivalent cycle of displacements of all or part of the basilar membrane from its resting or neutral position; and (2) that stimulation of the hair cells of the basilar membrane is due to this displacement of the membrane, and that, other things equal, the greater the displacement, the greater is the excitation effect produced in any hair cell in a displaced region of the membrane. These assumptions are implicit in many of the current discussions of the action of the cochlea.

The bearing of these assumptions on the question of asynchronism in the neural volleys may be illustrated in terms of two of the hypotheses used to account for an increase in the number of cells responding with increase in the intensity of the stimulating sound wave. One hypothesis (the "spread hypothesis") rests on the presumption that the extent of displacement along the *length* of the basilar membrane is greater for stimuli of greater amplitude. It is contended that more intense stimuli produce basilar displacements of greater linear extent, and thus lead to the stimulation of more cells. The second hypothesis (the "multiple limen hypothesis") suggests that not all the hair cells in a given region have the same limen. Sound waves of greater amplitude should stimulate more cells since the greater amplitude of displacement produced in the basilar membrane should lead cells of relatively less sensitivity to respond as well as those of maximal sensitivity.

Now if, in the case of either of these arrangements, the basilar membrane is followed through for an individual cycle, it becomes apparent that the constituent impulses in a volley will not be discharged synchronously but will, instead, be spread out in time. Suppose, for example, that cells of like sensitivity occur along the extent of the basilar membrane. Those which occur at the part of the membrane first undergoing displacement will necessarily be stimulated before those occurring in immediately adjacent regions. Consider, secondly, the consequences of the "multiple limen hypothesis." Suppose fibers of different limens occur at a region of the membrane undergoing displacement, and that these are all stimulated by the displacement. The more sensitive fibers must necessarily be stimulated before the less sensitive ones. For in the transition of the membrane from zero displacement to that necessary for stimulating the least sensitive of the fibers, all intermediate displacements must occur. So the fibers of different thresholds will discharge successively, in the order of their sensitivity, the most sensitive one discharging first.

An additional point of interest here relates to the phase position of the stimulating wave at which any given cell will discharge. It is apparent that the phase at which the cell will discharge must vary in a definite way with the amplitude of the wave for a stimulus of given frequency. For, the displacement of the basilar membrane necessary to excite the cell must be reached earlier in a wave of large amplitude

[32] G. Kreezer, A neglected possibility in frequency theories of hearing, this JOURNAL, 43, 1931, 659-664.

than in one of small amplitude. This point becomes obvious immediately if two sine waves of the same frequency but different amplitudes are compared. An ordinate of any given height occurs sooner in the cycle of the wave of large amplitude than it does in that of small amplitude.

One consequence of the hypothesis outlined above might be tested without any great difficulty. Records of the Davis-Saul effect obtained by means of a high frequency oscillograph, such as the cathode ray oscillograph would show whether, as the hypothesis suggests, the duration of the successive volleys increases as the amplitude of a stimulating sound wave of given frequency increases from liminal to maximally effective values.

Wever-Bray effect: (a) Cause. Before any significance which the Wever-Bray effect may have as an index of neural events can be determined, it is obvious that the phenomenon or phenomena on which it depends must first be discovered. As the problem stands at present, it is possible to conceive of any one or more of a considerable variety of bio-microphonic phenomena as involved in the effect.[33] But it would be valid to use the effect as an index of neural events only if it were due to a particular type of bio-microphonic phenomenon.

One of the possibilities that seemed at first to exist was that the Wever-Bray effect might be due to action currents of the auditory nerve. But this possibility has apparently been eliminated by the evidence that the action currents of the nerve give rise to the Davis-Saul effect. The afferent impulses in the nerve cannot be characterized by two contrary sets of properties. If the volleys of afferent impulses have properties like those described for the Davis-Saul effect, they cannot have properties like those characterizing the Wever-Bray effect.

One possibility remains, however, that would be of undoubted significance for auditory theory: the Wever-Bray effect may be due to action currents produced in the very earliest stage of neural conduction, that is to say, in the hair cells, regarded collectively. The available data on the Davis-Saul effect do not exclude this possibility; for these data link the Davis-Saul effect only with the afferent impulses in the auditory nerve and tracts. The nature of the action currents in the auditory receptors themselves, their frequency and form as a function of the stimulus, is still a problem for further research. These action currents may have the properties of the Davis-Saul effect; they may have the properties of the Wever-Bray effect, or they may have properties different from either.

There is one line of evidence, however, which makes it seem unlikely that the Wever-Bray effect can be due to action potentials in the hair cells.

[33] Cf. Kreezer, *op. cit.*, this JOURNAL, 44, 1932, 641-649.

A number of investigators, it will be recalled, reported that the Wever-Bray effect reproduces not only the frequency but also the *form* of the stimulating sound wave. Thus, Wever and Bray, Adrian, and Rademaker and Bergansius all indicate that when the stimulus is sinusoidal in form, the electrical effect detected is likewise sinusoidal.[34] Davis and Saul too report that, except for frequencies beneath 800 cycles, the "spread" reproduces the wave form of the stimulus.[35] For frequencies beneath 800 cycles, they state that the form may be somewhat more complex.[36]

Now none of the hypotheses proposed so far to account for the retention of the stimulus frequencies in the response of the nerve as a whole explains how the wave form of the stimulus might also be retained. The volley theories account only for the reproduction of the stimulus-frequency, but not for that of the form.[37] Surely, any neural explanation of the *form* of the Wever-Bray effect, to be adequate, must not only suggest a temporal order of occurrence of action potentials in different fibers capable of building up the form of the stimulus wave, but must also suggest a plausible way in which this temporal order might be produced in the cochlea by the action of the stimulus. The mechanism of the cochlea must be taken into account as well as the properties of the nerve cells. The present lack of such a hypothesis makes it difficult to believe that the Wever-Bray effect can be due to action potentials in the hair cells.

The methodological character of the problem involved in the determination of the properties of the action potentials in the hair cells deserves emphasis. So long as the possibility exists that sound waves transmitted to the labyrinth fluids may give rise to a number of *different* electrical phenomena, the problem of determining the properties of the action potentials in the hair cells becomes one of developing a technique by means of which these potentials can be *isolated*. A similar problem was involved in the determination of the properties of the action potentials of the nerve. This problem Davis and Saul were apparently able to solve by an appropriate control of stimulus-intensity, the degree of amplification, and the position and character of the electrodes. In the case of the auditory receptor cells, analogous means must be developed for isolating the action potentials in order that

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[34] See footnote 18.

[35] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 359.

[36] This additional complexity in form may possibly be due to harmonics introduced into the stimulating sound waves as a result of the non-linear transmission characteristic of the ossicles. The question cannot, however, be settled without quantitative data. Cf. G. Kreezer, *op. cit.*, this JOURNAL, 44, 1932, 674.

[37] Cf. Troland, *op. cit., J. Gen. Psychol.*, 2, 1929, 43 f., 50 f.; Wever and Bray, *op. cit., Psychol. Rev.*, 37, 1930, 375-379.

their properties may be determined, unobscured by the properties of other phenomena which might occur simultaneously.

(b) Its use as an index of sound transmission in the ear. Our previous discussion has been concerned with the possible significance of the auditory electrical effects as indicators of neural processes. Another possibility of importance is that the effects may also be capable of use as indicators of events in the physical stage of the hearing process; that is to say, they may be able to throw light on the intensity of sound transmitted as stimuli to the inner ear, and on the function of various parts of the ear in regulating this transmission.

An attempt to use the Wever-Bray effect as an index of the intensity of the sound waves transmitted to the cochlea has been made by Hughson and Crowe.[38] As a result of their experiments, these authors have proposed a new hypothesis concerning the function of the round-window membrane. Finding that pressure on the membrane led to an increase in the intensity of the Wever-Bray effect, they concluded: (1) that the increased pressure must have led to an increase in the intensity of the sound stimuli reaching the cochlea; (2) that the round-window membrane, owing to its mobility, absorbs a large percentage of all sound entering the ear; and (3) that this membrane acts as a safety valve to protect the structures of the inner ear. In their recent experiments with the Wever-Bray effect, Hughson and Crowe have placed emphasis upon the development of more precise methods for measuring the intensity of the effect as detected in the telephone or loud speaker.[39] The weakness in their whole position lies, however, not in the lack of sufficient precision in their observations, but in the failure to prove that the Wever-Bray effect may be accepted as a reliable indicator of the intensity of the sound waves effective as stimuli in the cochlea. In the absence of such proof, the observations of Hughson and Crowe on the changes produced in the Wever-Bray effect by experimental modifications of the round-window membrane cannot be regarded as lending support to their hypothesis concerning the functions of this membrane. Their hypothesis, moreover, receives no support on the grounds that it represents the only possible explanation of their results. As we shall see below, other explanations may be given.

Justification for the use of a given effect α as an indicator of some variable might be claimed, conceivably on one of two grounds: (1) on the grounds merely of an empirical correlation between the properties of the effect and

[38] Hughson and Crowe, *op. cit., J. Amer. Med. Assoc.*, 96, 1931, 2027-2028.

[39] W. Hughson and S. J. Crowe, Experimental investigation of the physiology of the ear, *Acta Otolar.*, 18, 1933, 291-339.

those of the variable; or (2) on the basis of knowledge of the *phenomena* connecting the effect and the variable in question as well as knowledge of the relations existing between their respective properties. Thus, if the effect were known to depend on the variable x in a given way, or if the effect and the variable x both depended on the same phenomenon, the properties of the effect and the variable x would vary coördinately, other things equal. In the use of the one as an index of the other, it would be necessary, of course, that other things be kept equal, that is to say, it would be necessary that additional factors which might influence the properties of the effect or of the variable x be kept constant, or their influence allowed for by means of appropriate calculations.

The first method mentioned above for justifying the use of an effect as an indicator is that of establishing an empirical correlation between the properties of the effect and the variable in question. The usefulness of an indicator resting on such a basis is subject to certain limitations. For so long as the nature of the phenomena connecting the variable and the effect is unknown, there is never any assurance that the correlation will hold under conditions different from those under which it was established. In order, therefore, to permit the *general* use of a given effect as an index of some variable, it would seem necessary that the nature of the causal relation between the two be known.

This point may be illustrated by reference to a hypothesis[40] which we have suggested to account for the observations of Hughson and Crowe without accepting their conclusions concerning the function of the round-window membrane. Suppose it is granted that sound energy transmitted to the labyrinth liquids by the vibration of the base of the stapes will be propagated in all directions through the liquids in accordance with the usual factors determining sound propagation. One channel for the sound waves will be through the cochlea by way of the basilar membrane and out through the round window membrane. Another channel will be through the cochlea by way of the helicotrema and out through the round window. A third channel will include all the paths by which sound waves may be transmitted through the liquids to the bone walls of the labyrinth and out through the walls to the tissues beyond. These three channels must be regarded as in parallel with each other, in the sense in which this expression is used in describing electrical systems. Further, we may assume that only the sound travelling in the first channel, via the basilar membrane, can be effective in stimulating the receptor cells in the organ of Corti, which rests on the basilar membrane. Suppose, secondly, that the sound energy transmitted through the walls of the labyrinth leads to a biomicrophonic effect which we may call the α effect, having properties like those of the Wever-Bray effect, and that the intensity of this effect varies with the intensity of the sound waves thus transmitted through the walls (in Channel 3). This

[40] Cf. Kreezer, *op. cit.*, this JOURNAL, 44, 1932, 671 f.

bio-microphonic effect might be thought of as produced in any or all of three possible ways: (1) as an electrical effect produced in the tissues upon the passage of the mechanical sound waves through them;[41] (2) as a resistance microphonic effect at the electrodes;[42] or (3) as a condenser-microphonic effect at the electrodes.[43] Let it be assumed, further, that no part of the α effect is due to action currents in the hair cells.

Let us now examine the conditions under which a correspondence would be found between the α effect and the intensity of the sound waves transmitted via the basilar membrane. Consider, first, what the effect would be of varying the intensity of the sound waves sent into the ear. Corresponding to such a variation, there would occur a variation in the same direction, both of the intensity of the sound waves transmitted via the basilar membrane, and in the intensity of the sound transmitted through the walls, and in consequence, a perfect correlation between the intensity of the sound transmitted via basilar membrane and the intensity of the α effect. Secondly, suppose various operations were performed upon the ossicles such as were performed by Hughson and Crowe in their experiments.[44] There would be a consequent decrease in the efficiency of the bones in transmitting sound vibrations, and therefore, for a given intensity of physical stimulus, a decrease both in the energy transmitted via the basilar membrane in unit-time and in that transmitted through the walls. Thus again a correlation in the intensity of the sound transmitted via basilar membrane and in the intensity of the α effect would be found. Thirdly, suppose the round window membrane (or some part of the walls) is punctured so as to lead to a loss of labyrinth fluid (as in certain of Hughson and Crowe's experiments).[45] The loss of the liquid would reduce the efficiency with which sound waves were transmitted by means of the liquid to the walls or through the cochlea. There would consequently be a decrease in the intensity of the α effect as well as in the intensity of the sound waves transmitted via the basilar membrane. Again a correlation between the α effect and the intensity of the sound waves transmitted via basilar membrane.

Suppose now, in an animal with intact labyrinth, pressure were exerted on the round window membrane, so as to increase the acoustic impedance of the cochlear channels to the range of stimulus frequencies being used. The intensity of the sound waves transmitted through the cochlear channels would accordingly decrease. *On the other hand,* since the alternating pressures set up by the stapes at the oval window could not now be reduced by the displacement of the cochlear fluids made possible by the yielding of the round window membrane, *more* acoustic energy would flow *through the walls* in unit-time.[46] There would thus be an *increase* in the intensity of the α effect, although at the same time a *decrease* in the intensity of the sound waves transmitted via basilar membrane. In this case, then, the α effect

[41] *Ibid.,* 646-648. [42] *Ibid.,* 644-646. [43] *Ibid.*

[44] Hughson and Crowe, *op. cit., J. Amer. Med., Assoc.,* 96, 1931, 2027.

[45] *Ibid.,* 2028.

[46] This type of behavior might be illustrated by considering how the amount of swelling of the walls of a rubber tube into which air is forced at a given pressure depends on the elasticity of the membrane closing the opposite end of the tube. The swelling of the walls would obviously be much greater when a rigid cap is used than when an easily stretched membrane is used to close the end of the tube.

would not be reliable as an index of the intensity of the sound waves transmitted via the basilar membrane.

It is apparent that although a perfect correlation might exist between the effect and the variable x for a number of conditions, this fact would not assure the existence of the correlation for still other conditions. To determine whether an effect might be used as an indicator under conditions different from those under which a correlation had been established, it would be necessary for the phenomena connecting the effect and the variable to be known.

An examination of the present status of the Wever-Bray effect, in the light of these considerations, indicates its inappropriateness as a general index of the intensity of the sound waves effective as a stimulus in the inner ear. The work of Davis and Saul shows that the Wever-Bray effect cannot be due to action currents of the auditory nerve. Whether the effect can be due to action potentials of the auditory receptor cells remains a question for further research. For the effect to be reliable as an indicator, moreover, it would not be sufficient merely for some component of the effect to be due to action potentials of the hair cells. It would be necessary also that other possible factors in the magnitude of the effect, such as bio-microphonic phenomena of the sort previously mentioned, should be under control so that they could either be eliminated or kept from varying independently of the component due to the hair cells.

The difficulties standing in the way of the use of the Wever-Bray effect as an index of cochlear events do not exist in the case of the Davis-Saul effect. The discoverers of the Davis-Saul effect have, as we have seen, presented strong evidence that the effect is due to action potentials of the auditory nerve. They have developed a technique for isolating the effect which permits its properties to be determined without masking by the Wever-Bray effect. In a particular experimental-animal, there should be no difficulty in maintaining sufficient precautions against the occurrence of variations in the excitability of the auditory receptors or nerve fibers, variations which would vitiate the reliability of the effect as an index of the intensity of the stimulus in the inner ear. Some additional work must be done, it is true, to determine the precise attribute of the Davis-Saul effect which is correlated with the intensity of the physical stimulus. As has been suggested, it is conceivable, on the basis of the data so far available, that the impulses in each of the successive volleys are asynchronous, and that the duration of the nerve volleys may vary with the intensity of the stimulating wave. In such a case, the total area under the curve representing any volley would have to be

taken as the index of the intensity of the stimulus rather than the height of the curve. On the other hand, if the impulses contributing to a given volley should be found to be synchronous, then for a given frequency, the height of the curve could be considered the appropriate index. There is every reason to expect that, after such preliminary correlations, the Davis-Saul effect might be used as a reliable index of the intensity of the sound waves effective as a stimulus in the inner ear.

Summary

The aim of the present paper was to discuss the significance of the auditory electrical effects for auditory theory. Two distinct effects have so far been reported in the literature: the Wever-Bray effect and the Davis-Saul effect. There are two circumstances under which such effects might be considered of significance for auditory theory: (1) if the effect could be considered an index of *neural* events occurring at some stage of the auditory pathways; and (2) if the effect could be considered an index of events in the *physical* stage of the hearing process, that is to say, an index of the sound waves serving as stimulus in the inner ear.

The possibility of using an effect as an index of neural events depends on the strength of the evidence that the effect is due to action potentials occurring at some level in the auditory pathways. New data recently brought forward by Davis and Saul seem to show conclusively that the effect discovered by them is due to action potentials of the auditory nerve and tracts. Consequently their report that the frequency-correlation between the stimulus and the volleys produced by the nerve as a whole does not extend beyond frequencies of 1000 per sec. indicates the incorrectness of the so-called *volley theories* of hearing. For these theories assume that the higher audible frequencies (*e.g.* those beyond 1000 cycles) can be transmitted as volleys of the auditory nerve as a whole. The data concerning the frequency limitation of the volleys from the whole nerve do not, however, throw any additional light on the correctness or incorrectness of the telephone type of auditory theory, which assumes that the entire range of audible frequencies are transmitted by the individual fibers of the nerve. This indecisiveness rests on the difficulty of obtaining information about the properties of the individual fibers from records of the volleys produced by the nerve as a whole. A number of additional problems relating to the properties of the individual fibers of the auditory nerve are discussed.

With respect to the Wever-Bray effect, the evidence that the Davis-Saul effect is due to action potentials of the auditory nerve and tracts eliminates the possibility that the Wever-Bray effect can be caused by action potentials

of these tissues. The possibility still remains that the Wever-Bray effect may be due to action potentials of the auditory receptor cells. There are facts which make this seem unlikely, but the question remains for further research to decide.

The present lack of knowledge as to the specific phenomenon or phenomena responsible for the Wever-Bray effect also makes it impossible to consider it reliable as a general indicator of the intensity of the sound-waves effective as stimuli in the cochlea. Consequently, the observations of Hughson and Crowe on the changes produced in the Wever-Bray effect by experimental operations upon the round window membrane cannot be regarded as lending support to their hypothesis concerning the functions of this membrane. Nor does their hypothesis receive support on the grounds that it furnishes the only possible explanation of their results, for other explanations can be given. The difficulties which face the use of the Wever-Bray effect as an indicator do not exist in the case of the Davis-Saul effect. It is accordingly suggested that it might be used as a reliable index of events at the physical stage of the hearing process.

RETROACTIVE INHIBITION IN THE LEARNING OF POETRY

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The phenomenon of retroactive inhibition has become, long since, a securely established fact; and as knowledge of the characteristics and conditions of such inhibition accumulates, it is becoming increasingly probable that forgetting is to a large extent a function of the events interpolated between learning and the measurement of retention.[1] The major experimental investigations of the problem have, however, almost always employed, as learning materials, series of disparate items having either very little meaningful relation to each other or none at all. It is a justifiable inference that, if interpolated events produce the forgetting of series of this kind which, by the time they have been learned, have been woven by the *S* into a partially meaningful organization, interpolated events will also produce the forgetting of materials which are meaningful to the *S* at the beginning of the learning period. This inference remains, however, at the level of hypothesis, and a more complete evaluation of the theory that all forgetting is a function of interpolated events waits upon experimental tests with a wide sample of representative meaningful materials. The present paper reports an experiment on the influence of interpolated learning upon the retention of a section of one representative sample, viz., poetry.

A brief review of the major learning materials previously employed in experiments upon retroactive inhibition will serve to place the present experiment in its proper context and perspective. In the work which gave impetus to all later studies of the problem, Müller and Pilzecker[2] used nonsense syllables and a number of

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[1] McGeoch has shown that disuse, as such, is of no significance as a primary conditioning factor of forgetting, and that interpolated events are the most important one of the two primary factors of which forgetting is a function. See J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Bull.*, 28, 1931, 670; and the complete paper of the same title, *Psychol. Rev.*, 39, 1932, 352-370.

[2] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Erg. Bd. 1, 1900, 1-300.

later investigators have followed this precedent.[3] Lists of meaningful words come second in frequency of use,[4] and after them lists of numbers,[5] memory spans of consonants and digits,[6] lists of figures of varying design,[7] chess patterns,[8] and mazes.[9] It is difficult, on account of the widely different experimental conditions under which these materials have been used, to make valid comparisons of the relative susceptibilities of the different materials themselves to retroaction. The fact of chief importance for the present purpose is, however, that all have been shown to be susceptible to some degree of disintegration from interpolated learning. With the possible exception of chess patterns, moreover, all are series of items which possess for the learner no large amount of initial organization or meaningful interrelation. There are no facts available which constitute direct evidence of the susceptibility of materials possessing such meaningful interrelation or unity to retroactive inhibition.

METHOD

Experiment I. In this experiment 12 lines (3 stanzas) from each of two different poems have been employed as the materials for original learning. The poems are *John Gilpin's Ride* (Poem I) and *Alice Brand* (Poem II) and the stanzas used are from those given in the Foster and Tinker Manual.[10] The experiment has been conducted with groups of *S*s and, in order to prevent the *S*s from copying from each other, one poem was used with those in the odd-numbered seats and the other with those in the even-numbered seats. This precaution, together with the close watch maintained by *E* during the writing of the recalls, has reduced the variable

[3] M. Foucault, Introduction à la psychologie de la perception. Expériences sur l'oubli ou sur l'inhibition régressive, *Rev. des Cours et Conférences,* 21, 1913, 444-454; R. Heine, Über Wiedererkennen und rückwirkende Hemmung, *Zsch. f. Psychol.,* 68, 1914, 161-236; J. E. DeCamp, A study of retroactive inhibition, *Psychol. Monog.,* 19, 1915, (no. 84), 1-69; E. C. Tolman, Retroactive inhibition as affected by conditions of learning, *Psychol. Monog.,* 25, 1917 (no. 107), 1-50; E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.,* 28, 1920, (no. 128), 1-57; J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612; E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.,* 34, 1925, (no. 161), 1-60; A. Dahl, Über den Einfluss des Schlafens auf das Wiedererkennen, *Psychol. Forsch.,* 11, 1928, 290-301; N. Y. Cheng, Retroactive effect and degree of similarity, *J. Exper. Psychol.,* 12, 1929, 444-449; J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262; The influence of degree of interpolated learning upon retroactive inhibition, *ibid.,* 44, 1932, 695-708; L. M. Harden, The effect of emotional reactions upon retention, *J. Gen. Psychol.,* 3, 1930, 197-220.

[4] W. Hinrich, Zur Frage der rückwirkenden Hemmung, *Zsch. f. Psychol.,* 99, 1926, 389-392; P. L. Whitely, The dependence of learning and recall upon prior intellectual activities, *J. Exper. Psychol.,* 10, 1927, 489-508; J. A. McGeoch and W. T. McDonald, Meaningful relation and retroactive inhibition, this JOURNAL, 43, 1931, 579-588; Tolman, Skaggs, and Harden, *opp. citt.*

[5] Tolman, Robinson, and Harden, *opp. citt.*

[6] E. S. Robinson, The "similarity" factor in retroaction, this JOURNAL, 39, 1927, 297-312; and L. M. Harden, A quantitative study of the similarity factor in retroactive inhibition, *J. Gen. Psychol.,* 2, 1929, 421-430.

[7] Dahl, *op. cit.*

[8] DeCamp, Robinson, and Skaggs, *opp. citt.*

[9] L. W. Webb, Transfer of training and retroaction, *Psychol. Monog.,* 24, 1917, (no. 104), 1-90.

[10] W. S. Foster and M. Tinker, *Manual for Instructors: Experiments in Psychology,* 1929.

of copying very close to zero. Each section of 12 lines was presented to *S* mimeographed on a single sheet and 5 min. were allowed for study.[11] Immediately following this period of study, *S* wrote a recall for which 5 min. were allowed. After 15 min. from the end of this recall period *S* wrote a second recall, for which 5 min. were given. One week later, at the same hour, a third 5-min. recall was written. This procedure was a constant for all groups and under all conditions.

The variables under experimental control are, of course, the interpolated events which fill the first 15-min. interval. In the control or rest condition the *Ss* took the Seashore pitch discrimination test for 15 min., writing their judgments on a previously prepared record sheet. This is far from a 'rest' condition in the sense of complete relaxation, but it clearly requires no learning activity, the factor which we wish to eliminate in this condition. One must use some device to prevent rehearsal by untrained *Ss* during the control interval. In the first work condition, *S* began to study another 12-line section of the same poem immediately following the first recall of the original material. He studied this interpolated poem for 5 min., immediately wrote for 5 min. a recall thereof, and then took for 5 min. the Seashore pitch discrimination test. This condition differs from the rest condition only in that the *Ss* studied an additional 12 lines of poetry and recalled them during the first 10 min. of the 15-min. interval. Under the second work condition, immediately after the first recall of the original material *S* began to study two lists of 10 nonsense syllables each.[12] At the end of 5 min. he was allowed 5 min. for recall, after which he took the Seashore test for 5 min. The variable here is, of course, the learning and recall of the nonsense syllables.

A clear understanding of the conditions will be facilitated by the following outline in which each step in order is enumerated. The initials OM and IM refer to the original and interpolated materials, respectively.

Rest Condition	Work Conditions 1 and 2[13]
Study OM 5 min.	Study OM 5 min.
Recall OM 5 min.	Recall OM 5 min.
Pitch Test 15 min.	Study IM 5 min.
Recall OM second time 5 min.	Recall IM 5 min.
7-day Interval	Pitch Test 5 min.
Recall OM third time 5 min.	Recall OM second time 5 min.
	7-day Interval
	Recall OM third time 5 min.

[11] The instructions for the study of the poetry were: "On the other side of this sheet you will find 3 stanzas of poetry. You will be given 5 min. in which to study them. Use any method you wish, but be sure to have studied each stanza about equally by the end of the 5 min. period. Waste no time, but study the stanzas with all the concentration which you can command."

[12] The instructions for studying the nonsense syllables were: "On the small strip of paper are 2 lists of nonsense syllables. You will be given 5 min. in which to study them. Spend the first 2 min. on the first list and when the signal is given go to the second list, and spend 2 min. on it. When a second signal is given review both lists for 1 min." All instructions were read twice to the *Ss* slowly and deliberately.

[13] The two work conditions are exactly the same in outline. In the first the interpolated material consists of 12 more lines of the same poem as the one which constituted the original material. In the second the interpolated material is 20 nonsense syllables in two lists of 10 each. All *Ss* learned the same syllables.

At the beginning of the experiment with each group each *S* was presented with all of the material which he would need, including blank sheets for the writing of the recalls, clipped together in order, with the proper designation written on the back of each sheet. At each successive step *S* had only to place the sheet at which he had been working face down at the bottom of his packet of material, turn over the next sheet at the proper signal, and proceed. It was possible, thus, to adhere rigorously to the time schedule planned. At the close of the second recall the *S*s were given careful and rigid instructions against rehearsal. They were also asked to record whether they had ever seen or heard the poems before; and the records of those who reported that they had were discarded.

The recalls of the poetry were scored in three ways: (1) number of lines correctly recalled, which is the score upon which most emphasis will be placed, since it is the most exact and rigorous measure; (2) number of lines the meaning of which is correct; and (3) number of lines the meaning of which is partially correct. In both (2) and (3) objective criteria were applied. A line was placed under (2) only when synonyms were substituted for the original words or when very slight rearrangements of words left the meaning intact. A line was included under (3) if 3 or more connected words, carrying part of the meaning of the line, were given. The nonsense syllables interpolated in the second work condition were scored in terms of number correct regardless of position.

In computing the degree of retroactive inhibition, it is necessary to deal with differences between percentages of recall rather than with differences between mean scores at the delayed recalls, on account of the differences obtaining between degrees of learning as measured by immediate recall. It is possible to compute percentages of recall either in terms of the raw scores or in terms of the means of the percentages yielded by each individual *S*. In this experiment the two methods yield results which are the same in tendency and the results from the former only will be presented. The latter has the advantage that sigmas may be found directly in terms of percentage of recall, but in our data a few cases yielded very high percentages of recall, thus distorting both means and sigmas to a degree which is misleading if the percentages are computed individually instead of from the means. A typical case of distortion occurs when *S* has scores on immediate recall of one line correct, 8 correct as to sense, and 2 partially correct, while on a delayed recall he has 3 lines correct, 4 correct as to sense and 3 partially correct. It is easily understandable that two of the lines which were correct only in meaning on the first recall should be correct in wording also at the second recall without assuming rehearsal,[14] but such a *S* would have a recall of 300%, a huge deviation from the mean of the individual recall scores. The use of the raw scores in such cases more conservatively and accurately represents the facts.

Experiment II. The rest and one work condition, with interpolated learning of poetry in the latter, have been repeated with different groups but under conditions duplicating those of Experiment I save that the recall after 15 min. has been omitted from both conditions. We are, thus, able to find by a comparison of the recalls after 7 days in the two experiments whether the distributed practice afforded by the recall after 15 min. has affected the degree of inhibition after 7 days.

[14] Cf. W. Brown, To what extent is retention measured by a single recall? *J. Exper. Psychol.*, 6, 1923, 377-382.

RESULTS

Experiment I. The data for each of the two poems used will be presented separately. This will not only permit comparison of the two, but, since the results are confirmatory of each other, will give increased assurance to the conclusions. Table I gives the mean numbers of lines correctly recalled, the sigmas of the means, the percentages of recall computed from these means, together with the percentages of retroactive inhibition found by subtracting the percentage recalled under work from the percentage recalled under rest and dividing the remainder by the percentage under rest. Percentage of recall is always found with the mean

TABLE I

LINES CORRECTLY RECALLED: MEANS, SIGMAS, PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

			Time of Recall						
			Immediately	After 15 min.			After 7 days		
Poem	Condition	N	Mean	Mean	% Rec.	% R.I.	Mean	% Rec.	% R.I.
I	Rest	35	8.65 ± .57	7.43 ± .62	85.9		5.08 ± .65	58.7	
	Work (Poet.)	31	6.96 ± .58	5.84 ± .60	83.9	2.3	3.84 ± .66	55.1	6.1
	Work (N.S.)	27	8.07 ± .49	7.11 ± .60	88.1	+2.5	3.85 ± .71	47.7	18.7
II	Rest	30	7.63 ± .61	7.03 ± .64	92.1		5.96 ± .64	78.1	
	Work (Poet.)	37	7.16 ± .50	6.19 ± .51	86.4	6.1	4.24 ± .55	59.2	24.2
	Work (N.S.)	28	7.00 ± .58	5.64 ± .60	80.5	12.5	4.39 ± .50	62.7	19.7

at immediate recall, which is thus treated as a measure of learning, in the denominator.

Recall after 15 min. is high under all of the conditions studied and there is only a very slight amount of retroaction present. When poetry is the interpolated material, 2.3 and 6.1% of inhibition appears. When nonsense syllables are the interpolated material, the first poem shows 2.5% better retention than under rest, and the second shows 12.5% of loss.[15] The differences between the means of the rest and work conditions, both at immediate recall and after 15 min., are not statistically reliable, and the means of the percentages of recall computed for each *S*, while showing the same tendency, lack statistical reliability. It may be concluded that the interpolated learning of 12 additional lines of the same poem decreases after 15 min. the recall of the 12 lines first learned by very small and sta-

[15] The interpolated poems gave immediate recalls, lines correct, of 7.5±.58 and 7.7±.46, respectively. The corresponding recalls of the nonsense syllables under the second work conditions were 8.5±.71 and 9.8±.94.

tistically unreliable amounts. The fact that this small amount of inhibition occurs with each poem, however, lends support to the reliability of the tendency.

When, however, 7 days elapse between the two recalls, the percentages of retroaction in the first work condition increase to 6.1 and 24.2. The differences involved are still not statistically reliable, but the agreement of the two poems weighs in favor of a tendency toward an increase in inhibitory effects with an increase in the elapsed time.

The interpolation of the learning of 20 nonsense syllables is followed by a slight facilitation effect in the case of the first poem and by 12.5% of inhibition in the case of the second. The lack of agreement makes interpretation difficult. It may be that degree of retroaction is a function of the poem used, particularly of its difficulty. Clearly the second poem tends to be learned somewhat less well and to have the greater susceptibility to inhibition. The lessened recall which appears after the interpolation of nonsense syllables may be valid.

TABLE II

LINES WITH MEANING WHOLLY CORRECT AND LINES PARTIALLY CORRECT: PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

			Meaning Correct				Meaning Partially Correct			
			After 15 min.		After 7 days		After 15 min.		After 7 days	
Poem	Condition	N	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.
I	Rest	35	125.3		135.7		208.3		170.8	
	Work (Poet.)	31	104.1	16.9	111.8	17.6	143.7	31.0	108.4	36.5
	Work (N.S.)	27	119.8	4.3	98.8	27.1	145.6	30.1	177.7	4.0
II	Rest	30	125.0		101.8		103.7		172.6	
	Work (Poet.)	37	112.0	10.4	95.0	6.6	121.4	+17.0	123.7	28.3
	Work (N.S.)	28	224.8	+79.8	200.0	+96.4	86.5	16.5	130.4	24.4

More important, however, is the fact of complete agreement in the increase in retroaction at the 7-day interval. Both poems, under both conditions of interpolation, yield considerably greater percentages of inhibition at the longer interval and the amounts following the interpolation of the syllables are not consistently larger or smaller than those following the interpolation of the poetry.

We shall inquire next into the influence of interpolated learning upon the two other scores, viz., lines correct in meaning but not wholly correct in verbal statement and lines partially correct in meaning. These scores are of secondary importance and it will be sufficient to present only the percentages of retention and of retroaction (Table II). There is in most

cases a gain in these scores from the immediate to the delayed recalls which represents, presumably, a shift from complete toward partial correctness. This shift occurs more often and usually to a greater degree under the rest condition than under the work conditions. This brings it about that, in lines correct in meaning, the interpolated learning of poetry produces 16.9 and 10.4% of inhibition after 15 min., and 17.6 and 6.6% after 7 days. The interpolated learning of nonsense syllables is followed by 4.3% of inhibition in the case of Poem I and by a facilitation effect of 79.8% with Poem II. After 7 days these figures have shifted still farther in their respective directions. In lines partially correct, Poem I suffers a considerable degree of inhibition from both interpolations after the 15-min. period and a still greater amount from interpolated poetry after 7 days. After this interval the inhibition from the syllables is smaller than it was after 15 min. Poem II is recalled better after the learning of the interpolated poetry than after the non-learning (rest) period and worse after the learning of the syllables. In both cases inhibition is relatively large after 7 days.

In 5 cases out of 6 the interpolated learning of poetry has produced at least a small amount of inhibition at recall after 7 days. It is necessary to conclude that poetry is, under these conditions, slightly susceptible to retroactive inhibition after a 15-min. interval and that these inhibitory effects become more pronounced after 7 days. In 4 cases out of 6 the interpolated learning of nonsense syllables is followed after 15 min., likewise, by decreased recall of the original material, and in the same percentage of cases this decrease becomes more marked after 7 days. Poetry is, thus, also susceptible to inhibition from the interpolated learning of nonsense syllables and this inhibition is of the same order of magnitude as that produced by the interpolation of lines from the same poem.

The conclusions just drawn are supported more clearly by the data in terms of lines wholly correct, which is the most rigid and directly interpretable method of scoring, than by the two secondary methods. The exact significance of these latter methods is less easy to see on account of the complex relationships between them and between each of them and lines totally correct. In general, however, they support the conclusions drawn from the primary method. The retroaction which occurs in terms of perfect lines probably operates to produce more partial scores and, since the partial scores (meaning correct and meaning partially correct) also suffer retroaction, the effect has clearly been cumulative in character, much greater than that which is represented in any single score, and, in one sense, the sum of the effects produced in all three. It follows that the conclusions drawn may be accepted with confidence.

Experiment II. This experiment is an exact reproduction of the rest condition and of the first work condition of the preceding experiment, save for the single exception that the recall after 15 min. has been omitted under both conditions. It is possible that the increased retroaction after 7 days may have had this recall as one of its major conditions; or, as is equally possible, the recall after 15 min. may have acted in the opposite direction to diminish the amount of the increment in retroaction. This experiment is designed to discover whether either of these possibilities is actual and whether the recall under discussion has had any important effect upon degree of retroaction after 7 days.

The results from the primary method of scoring appear in Table III. The percentages of inhibition are larger, in the case of each poem, than

TABLE III

LINES CORRECT: MEANS, SIGMAS, PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION[16]

	Poem I					Poem II				
Condition	N	Immed. Rec.	Rec. 7 days	% Rec.	% R.I.	N	Immed. Rec.	Rec. 7 days	% Rec.	% R.I.
Rest	33	7.84±.60	4.45±.58	56.7		31	6.42±.54	3.29±.63	51.2	
Work	31	8.29±.59	3.35±.64	40.2	29.1	30	6.30±.50	2.00±.49	31.7	38.0

are the corresponding percentages found in Experiment I. They are 29.1 and 38.0 as compared with 6.1 and 24.2. The means of the immediate recalls of the two experiments are not as nearly identical as one could wish, but there is no apparent reason for assuming that the small differences present are the major conditions of the results. It may be concluded from the two experiments that the poems used are, at recall after 7 days, clearly susceptible to inhibition from the interpolated learning of similar poetry and that the degree of inhibition is greater when there is no intervening recall after 15 min. The recall after 15 min. operates to diminish the inhibition at the subsequent recall after 7 days.[17]

The secondary scores are shown in Table IV. Again only the percentages need be given. The percentages of retroaction in terms of meaning correct are 20.7 and 40.7 and are, thus, larger than those found in Experiment I. Lines partially correct show less inhibition for Poem I than in the preceding experiment and greater inhibition for Poem II. When it is noted

[16] The immediate recalls of the 12 lines of interpolated poetry are 8.03±.57 and 7.56±.53.

[17] The experiments afford no evidence upon the manner in which this recall operates to reduce inhibition at the subsequent recall, but it is a plausible speculation that it does so by virtue of the rehearsal or relearning which it provides.

that in this experiment Poem I suffers a considerably larger degree of inhibition of perfect lines than it did in Experiment I, 29.1% as against 6.1%, the failure of partial lines to follow the general tendency and to show an increased retroaction becomes inconsequential. With this single unimportant exception these secondary measures support the conclusion drawn from measurement in terms of lines perfectly recalled.

TABLE IV

LINES WITH MEANING WHOLLY CORRECT AND LINES PARTIALLY CORRECT: PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

Condition	Poem I				Poem II			
	Meaning Corr.		Partially Corr.		Meaning Corr.		Partially Corr.	
	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.
Rest	77.2		132.2		50.6		98.3	
Work	61.2	20.7	112.1	15.2	30.0	40.7	51.6	47.5

DISCUSSION

We have found that poetry is susceptible to a small degree of inhibition from the interpolated learning of poetry of the same kind, when recall occurs 15 min. after the original learning, that this susceptibility is increased considerably after 7 days, and that it is greater after 7 days when there has been no intermediate recall after 15 min. The interpolated learning of nonsense syllables has the same general effect as does the interpolation of poetry when recalls occur after 15 min. and again after 7 days. These results raise three questions which require discussion. (1) What is the importance of the fact that poetry is susceptible, even though only slightly when the interval is but 15 min., to a decrement from interpolated learning? (2) What interpretation is to be put upon the increase in degree of inhibition with increasing time interval? (3) In the light of the relation, demonstrated with other materials, between similarity and retroaction the rough equality between the amounts of inhibition following interpolations of poetry and of nonsense syllables is anomalous and calls for explanation. This question obviously involves the transfer and perseveration theories of retroactive inhibition. We shall deal with these three in order.

A major part of our problem was to discover whether poetry is at all susceptible to disintegration from interpolated learning and, thereby, to make one test of the hypothesis that interpolated experience is a major condition of forgetting. We have found that it is susceptible and have, to that extent, increased the generality of the hypothesis. The interpolated

learning does not, of course, account for all of the forgetting which has measurably taken place. In the rest condition, without interpolated learning, a decrement occurs; it is only less in extent than that found under work. This decrement under rest may be a function of the events which filled the interval—the process of discrimination, the inevitable flux of events arising from extra- and intra-organic stimulation, and from associative sequences. That everyday experience of this general kind has an inhibitory effect Jenkins and Dallenbach have shown,[18] and it is reasonable that the decrements under the rest condition, and those which remain over and above the effects of interpolated learning under the work condition, are a result of such experience. There is also the possibility that a blocking of perseveration and, perhaps, other factors have been operative. The important thing here is, however, that, whatever other conditions may also be present, poetry is susceptible to inhibition from interpolated learning and that, by virtue of this fact the generality of the phenomenon of retroaction is extended and its importance as a condition of forgetting thereby increased.

The fact of the slight amounts of inhibition after 15 min. offers a problem, also, but one which reaches beyond the boundaries of the present experiment and into the wider issues of mental organization and of psychological system. Into these issues we shall not venture. It is sufficient here to recall that the already available facts upon curves of retention lead one to expect a smaller decrement in the case of poetry than in that of the less organized serial materials and that the fact of the slight decrement from interpolation is consistent with these other facts. To ask why poetry should suffer so small a loss is tantamount to asking why meaningful material is forgotten less rapidly than is non-meaningful, an inquiry which points beyond the intent of our discussion.

We may now turn to the second problem, that of the interpretation of the increase in retroaction from 15 min. to 7 days. There are at least four conditions to be considered as possible factors in the bringing about of this result.

(a) It is possible that the increase after 7 days is a statistical artifact. This would be readily thinkable on some such grounds as the following. Let the interpolated learning inhibit the recall of one line of poetry after 15 min. and let this line be inhibited permanently so that it is not recalled again. Then let whatever other factors bring about forgetting operate equally upon the activities learned under rest and under work. The result would be a constant difference between the two conditions at successive points in time, but a steadily increasing percentage of in-

[18] Jenkins and Dallenbach, *op. cit.*

hibition as the absolute number of lines recalled under rest grows smaller. We would have, then, a constant amount of absolute decrement, which is converted into an increasing amount by our arithmetic. We may test the artifactual character of the increase in percentage of inhibition by examining the gross differences between the means for rest and work at the two delayed recalls. These differences should be, allowing for the inevitable margin of variation, the same. The differences between the means are (Experiment I) for Poem I, 1.59 and 1.24, and for Poem II, 0.84 and 1.72. But these figures are open to the variable of differences between groups and one should find the differences between the percentages which the recalls at 15 min. and 7 days are of the immediate recalls. These differences are 2.0 and 3.6 for Poem I and 5.7 and 18.9 for Poem II. There is nothing here, particularly in the data from Poem II, the one which shows most clearly an increase in retroaction with time, to support the suspicion of a constant difference, unless one is willing to admit a very wide range of variation under the concept of constancy. Since the differences are of uncertain reliability, a wide range must be allowed, but it must also be remembered that the obtained increases in percentage of inhibition have been found from the figures which yielded the above differences and that the consistency of direction presented by the percentages of inhibition does not follow the differences between the means, either of lines recalled or of percentages of recall. Nor can it be overlooked that, even should absolute differences remain the same, they would validly take on an increasing relational significance as the magnitude of the mean recalls decreases unless it could be shown that the constant absolute difference refers to the same items. As a matter of fact, there is no evidence, in the fluid and shifting picture which successive recalls present, that interpolation has permanently inhibited any particular parts.

If we grant that the result is valid, we may turn to three possible hypotheses concerning it.

(b) The first of these is that whatever conditions operate in the production of a decrement may continue to operate progressively, regardless of the cessation of the interpolated learning. This progressive operation would, of course, bring about increasing decrements with time. Such continued function long after the removal of the stimulus-conditions has, in so far as is known, no reportable associative aspects and, while such action is theoretically possible, it is hardly good theorizing to fall back upon assumed functions which are unspecified, as these must be, so long as hypotheses of greater explicitness are available.

(c) A second hypothesis is that the interpolated learning weakens the integration of the activities involved in the original learning, thus causing the latter to be more susceptible to the inhibitory effects of the succeeding experience of daily life than is the original material under the rest condition. The latter has, on account of the absence of interpolated learning, the advantage of a better organization, with the greater resistance to disintegration which such organization may be expected to provide. The small amount of inhibition after an interval of 15 min. seems, on the face of it, to discredit this hypothesis, since the integration of the original material, if weakened by the interpolated learning, should be much less after 15 min. in order that there should be a sufficient difference between the integrations of the original materials under rest and work to furnish two different degrees for everyday experience to work upon. This difficulty is less serious than it appears to

be, since it is possible that the interpolated learning may have disintegrated the organization of the original material to an extent which, while not sufficient to show greatly after so short a time as 15 min., renders it more open to inhibition from everyday experience. In order to account, on this basis, for the divergence between rest and work conditions, one has to assume, further, that inhibition from daily activities is cumulative in the sense that successive experiences find the integration less well knit and therefore weaken it still more. On the basis of such an hypothesis, positively accelerated curves of forgetting would be understandable. That such curves do not often appear may mean either that the hypothesis is wrong or that there are limiting conditions, as yet largely unknown, upon the distintegrating effect of interpolated experience as the original activity changes progressively through time.

(d) Still a third possibility is that the actual inhibition does not occur until the time of recall. During recall confusion or interference might take place between the original and interpolated materials, both of which might be reinstated, either wholly or in part, at that time. This hypothesis is at least as reasonable as is the preceding one in so far as the present results are concerned. According to it the very small degree of inhibition after 15 min. and the larger amount after 7 days could conceivably be a function of the relative degrees of organization of the two sections of poetry at the time of the recall of the original section.

Estimation of the validity of such an hypothesis as the last one, which is at least reasonable, is made more difficult by the fact that the results under discussion do not agree with those obtained from another experiment[19] in which percentages of inhibition computed separately after 20 min., and 1, 24, 48, and 144 hr. fail to show any clear change in magnitude with increasing time interval. This other experiment employed lists of 10 adjectives learned and relearned by the anticipation method, while the present experiment has used poetry learned by the method of complete presentation and recalled in writing at a single instigating cue. It may be that the differences in material and in method of learning are sufficient to account for the differences between the results, but there is no specific evidence that such is the case. It may also be the case that differences in relative degrees of learning are the important factors. Both experiments have been performed with as much care as we can exercise and it seems better to delay further attempts at interpretation of the results from the recalls of poetry until more can be learned about the modes in which retroactive inhibition operates.

The fact that the interpolated learning of nonsense syllables is followed by decrements similar in magnitude to those produced by the interpolated learning of poetry is a radical departure from the expectation raised by preceding work and offers a third problem for interpretation. It has frequently been shown that, within limits which are not yet empirically established, retroactive inhibition is inversely related to the degree of similarity between the original and interpolated materials.[20] Two 12-line sec-

[19] McGeoch, The temporal course of the inhibitory effects of interpolated learning, *J. Gen. Psychol.*, 9, 1933, 24-41.

[20] Cf. the papers, already cited, by Robinson (1920, 1927), Skaggs, Whitely, Cheng, Harden (1929), and McGeoch and McDonald.

tions from the same poem are, by any criterion, more similar to each other than are one such section and 20 nonsense syllables. The two sections of poetry are, moreover, sufficiently different to prevent any supposition that the learning of the second constituted a continued practice at the first and, thus, a situation from which inhibition could not be expected. It is, also, far from likely that the learning of the interpolated lines was accompanied by a running back over or rehearsal of the 12 lines previously learned. The rhythm of the two sections is the same, but the *S*s were not told that the two sections were from the same poem and the narrative is sufficiently loose to prevent ready detection of the fact that it is continued in the second section. The recall of the interpolated section was, furthermore, as good as that of the original one, a result hardly to be expected if time had been spent reviewing the original one. These facts, together with the additional one that the *S*s were learning under a time limit and would have no reason for going back to rehearse the poem previously learned and recalled, make the comparison of the results of the two work conditions valid. They offer, then, equivalent amounts of inhibition from the interpolation of two activities which differ very widely in their similarity to the original activity. These two interpolations have results of the same character after both 15 min. and 7 days.

This failure of our results to conform to the tendency of other materials offers, in itself, an important problem, at the solution of which one may now only guess. In experiments upon retroactive inhibition, connected and meaningful materials have been almost never studied and to what extent one is justified in expecting from them results which are directly comparable to those yielded by disparate series of items is unknown. Our results raise, however, two general questions concerning inhibition: (1) whether similarity is the major condition of its appearance, as the transfer theory indicates, and (2) what constitutes similarity, in the context of retroaction experiments. These are not new problems. They have been present in most of the previous work. Our results merely serve to extend them and to make them still more puzzling.

The fact that nonsense syllables are as potent to inhibit poetry as is more poetry of the same kind is a difficult one for the transfer theory as that theory is usually stated, since, if transfer is at the basis of retroaction, similarity and inhibition should, over a wide range, be positively correlated. It is probable that relative degrees of organization must be considered in the theory.

It may be that relative degrees of organization of the original and interpolated materials are very important in determining amount of retro-

active inhibition and that, when both materials possess a fairly high degree of internal organization, as is probably the case when both are sections of poetry, retroaction is, other things being equal, at a minimum. It may be, further, that a low degree of organization in the interpolated material, for example, permits a more ready confusion between it and the original material and hence a greater amount of inhibition. That is, degrees of organization may constitute a factor coordinate with similarity in an interpretation of the transfer theory. In this experiment the nonsense syllable lists were doubtless less well organized into a series than was the interpolated poetry and, as a result, they may have transferred more readily. If we score the nonsense syllables in terms of number correctly recalled and in correct position, the mean scores become less. This indicates a loose internal organization, although such proof is scarcely needed to show that syllable lists do not have as closely knit a pattern, even when well learned, as does poetry. The factor of degree of organization may have, in the present experiment, obscured differences produced by similarity.[21]

On the other hand, the perseveration theory is favored by this result. It is an essential part of this theory that the important condition of retroactive inhibition is the diminution or complete blocking of perseveration by the interpolated activity. The crucial aspect of this activity should be its intensity, while its similarity to the original activity should be inconsequential. According to this theory, therefore, the interpolated learning of nonsense syllables might be expected to exercise an inhibitory effect fully comparable to that produced by the interpolated learning of poetry. It is believed, however, that before this apparent contradiction of the transfer theory and support of perseveration may be given adequate evaluation, both theories, and more particularly the transfer theory, must be more carefully analyzed. Until more experimental work, directed at such an analysis, has been done, theorizing is futile.

Conclusions

(1) When a section of the same poem as the one used for original learning is employed as interpolated material, small amounts of retroactive inhibition occur in recall after 15 min. Susceptibility to inhibition is considerably greater after 7 days than after 15 min., and is enhanced when there is no intermediate recall after 15 min.

[21] We are indebted to Dr. Arthur W. Melton, of Yale University, for the suggestion that differences in the organization of the materials may obscure differences in inhibition resulting from similarity.

(2) The use of nonsense syllables as material for interpolated learning produces roughly the same amount of inhibition as does the interpolation of poetry. This fact, as it stands, favors the perseveration theory and contradicts the expectation from the usual statements of the transfer theory. Before, however, the results can be confidently interpreted, both theories require further analysis.

(3) The fact that poetry is susceptible to a decrement from interpolated learning increases the generality of this decrement-phenomenon and adds support to the theory that retroactive inhibition is a major condition of forgetting.

THE AFTER-EFFECT OF VISUALLY OBSERVED MOVEMENT

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The present paper records the results of certain experiments upon the after-effect of visually observed movement.[1] After-effects of certain forms of visually observed movement have long been noted, especially those which give the appearance of movement in the opposite direction, or in some other way opposed to the previously observed stimulus-movement.

HISTORY

The experimental investigation of this phenomenon has been undertaken by physicists, physiologists, and psychologists. The history of this work from the first observation by Aristotle up to 1911 has been given by A. Wohlgemuth.[2] This author summarizes the work of many other investigators.[3]

The most important work done upon this problem since 1911, the date of the publication of Wohlgemuth's monograph, may be summarized as follows:

Wohlgemuth's comprehensive investigation included the repetition of many previous experiments and the confirmation in general of the after-effect of "apparent movement in the opposite direction."[4] It included also some 34 additional experi-

* Accepted for publication October 15, 1932.

[1] The writer wishes to express her gratitude to Professor Leonard Carmichael of the Psychological Laboratory of Brown University under whose guidance this investigation was carried out.

[2] A. Wohlgemuth, On the after-effect of seen movement, *Brit. J. Psychol., Monog. Suppl.*, 1, 1911, 1-117.

[3] J. Plateau, Vierte Notiz über eine neue sonderbare Anwendung des Eindrücke auf der Netzhaut, *Pogg. Annalen*, 80, 1850, 287-292; H. K. Müller, Die Beobachtung von Tiefeneffekten bei die binokularen Bewegungnachbildern, *Zsch. f. Sinnesphysiol.* 59, 1928, 157-165; J. J. Oppel, Neue Beobachtungen und Versuche über eine eigentümliche noch wenig bekannte Reaktionstätigkeit des menschlichen Auges, *Pogg. Annalen*, 99, 1856, 540-561; V. Dvorak, Versuche über die Nachbilder von Reizveränderungen, *Ber. der Wien. Akad. der Wiss.*, 61, 1871, 257-262; *Helmholtz's Treatise on Physiological Optics*, trans. 3rd ed. by J. P. C. Southall, III, 1925, 248, 270-281; S. P. Thompson, Optical illusions of motion, *Brain*, 3, 1880, 289-298; Optical illusions of motion, *Pop. Sci. Mo.*, 18, 1881, 519-526; H. P. Bowditch and G. S. Hall, Optical illusions of motion, *J. Physiol.*, 3, 1881, 296-301; E. Budde, Ueber metakinetische Scheinbewegungen und über die Wahrnehmung von Bewegungen, *Arch. f. Anat. und Physiol.* (Dubois Reymond), 1884, 127-152; S. Exner, Einige Beobachtungen über Bewegungsnachbilder, *Centralbl. f. Physiol.*, 1, 1888, 135-140; Über optische Bewegungsempfindungen, *Biol. Centralbl.*, 8, 1888, No. 14; A. Borschke and L. Hescheles, Über Bewegungsnachbilder, *Zsch. f. Psychol.*, 27, 1902, 387-398; A. von Szily, Bewegungsnachbild und Bewegungskontrast, *ibid.*, 38, 1905, 81-154; Zum Studium des Bewegungsnachbildes, *Zsch. f. Sinnesphysiol.*, 42, 1908, 109-114; T. Kinoshita, Zur Kenntniss der negativen Bewegungsnachbilder, *ibid.*, 43, 1909, 420-433; Über die Dauer des negativen Bewegungsnachbildes, *ibid.*, 434-442.

[4] Wohlgemuth, *op. cit.*, 27.

ments, pertaining to the influence of such factors as brightness, extent, direction, and velocity of the stimulus-movement, and the portion of the visual field stimulated, upon the after-effect. An "apparatus of moving fields" was devised, which consisted of two horizontal parallel-stripe fields, permitting simultaneous presentation of two vertical movements in opposite directions. With the spiral disk Wohlgemuth found that colors of the same brightness produced the same after-effect; that green and yellow spirals gave a marked after-effect in dim as well as in bright light, showing the equal importance of rods and cones; and that the after-effect resulted even when the area of objective movement was nearly zero. An elaborate network of subcortical and cortical movement centers was suggested to explain the phenomenon.

Kinoshita's study of the duration of the movement after-effect was extended by Takei,[5] who found this feature of the effect to be definitely related to the color of the objective movement as well as to the speed, direction, and intensity of light in that movement. Using 8 colors of equal chroma and brilliance, he obtained the longest duration with yellow, and the shortest with violet, a result suggesting that the intrinsically more brilliant color may produce the longer duration.

Hunter found that the after-effect varied with the stimulus-object.[6] On the basis of this and other observations he maintained, therefore, that a single explanation cannot be given for all instances of the effect, but that the three following factors, variously combined, explain the several phenomena: (1) eye-muscle strain, due to the reflex tendency of the eye to follow the movement, (2) suggestion, an associative factor, and (3) a retinal factor, probably in the form of fading after-images on the retina. Certain observations which led to the postulation of these three factors were: the faintness of the phenomenon when observed in the unstimulated eye (due to the absence of a retinal factor), the voluntary control of the effect (due to association), and the inability of the subject to secure an after-effect after being trained to control it (due to habits of eye-strain set up in the process of control). The phenomenon of the spiral disk he considered, on the other hand, to be predominantly retinal in origin, since no transfer of the effect to the unstimulated eye took place, and since the observation of superimposed opposite movements yielded a normal and intense after-effect. The retinal factor is probably a streaming phenomenon moving in the opposite direction to that of the stimulus-movement.[7] Hunter, with his factor of suggestion, provides the interpretation which his retinal theory demands if it is to explain how after-images are seen as movement in a specific direction.

Wohlgemuth's statement that "no after-effect is produced by an objective movement occupying the whole visual field" has been refuted by Thalman.[8] Under conditions, at first identical with and later similar to Wohlgemuth's, he obtained a normal opposite after-effect. The conditioning factor in this case was the duration of the stimulus-movement and not the position of fixation or rate of objective movement.

In a brief summary Ehrenstein[9] attempted to refute Hunter and to support Szily

[5] T. Takei, Über die Dauer des negativen farbigen Bewegungsnachbildes, *Zsch. f. Sinnesphysiol.*, 47, 1913, 377-381.

[6] W. S. Hunter, The after-effect of visual motion, *Psychol. Rev.*, 21, 1914, 245-277.

[7] Hunter, Retinal factors in visual after-movement, *ibid.*, 22, 1915, 479-489.

[8] W. A. Thalman, The after-effect of seen movement when the whole visual field is filled by a moving stimulus, this JOURNAL, 32, 1921, 429-441.

[9] W. Ehrenstein, Versuche über die Beziehungen zwischen Bewegungs- und Gestaltwahrenehmungen, *Zsch. f. Psychol.*, 96, 1925, 305-352.

with positive results, which showed transfer of the effect, obtained with a logarithmic spiral, to the unstimulated eye. He also found the duration of the effect to be dependent upon the duration of the stimulus-movement.

Granit found a complex relation between the extent of the stimulus-movement and the duration of the effect.[10] When the extent was decreased by changing the size of the disk the duration decreased, but when it was decreased by increasing the observation-distance the duration increased slightly. Granit concluded that inhibitory activity of the cones upon the activity of the rods explains the phenomenon.

The following year Granit furnished further support for this view.[11] He found the optimal visual angle for the effect to be 2-4° involving an area which was practically rod-free. Moreover, the greatest decrease in duration occurred with a very slight degree of dark-adaptation, a result showing that the rods exerted their greatest inhibitory influence upon the cones without, however, completely overcoming them.

Fuchs has demonstrated the influence upon the movement after-effect of (1) the projection-figure, and (2) the projection ground.[12] Certain stimulus-figures, such as a picture of human beings walking, lent themselves particularly to the production of the phenomenon. A radii disk, projected after observing the spiral disk, had a tendency to show the radii as curved slightly. Fuchs concluded that the effect depends upon (1) a peripheral factor determining the direction of the stimulus-movement, and (2) a central, or perceptual factor, consisting in the "structure" of the projection-ground, and the influence of the observer's past experience with this projection-ground.

Müller observed certain depth effects as introspectively characteristic of the after-effect when projected either on a stationary, or moving, projection ground.[13]

One form of 'motor theory' based on the activity of the accommodation muscles has recently been proposed by Leiri to explain the spiral after-effect.[14] The phenomena of apparent increase and decrease in size of a visual object, known as macropsia and micropsia respectively, take place normally when a change in accommodation is produced by the approach or withdrawal of an object. In the case of the spiral the apparent approach produces, according to Leiri, an enlarged retinal image which becomes a stimulus for a reflex increase in accommodation. When the objective rotation ceases this stimulation is gone and the reflex activity stops. There continues, however, an "accommodation-innervation" which has been compensating for the reflex activity during rotation and which now produces the after-effect until the eye becomes adjusted to the new degree of accommodation. The theory, if correct, explains a transfer of the effect to the unstimulated eye, since the two eyes are closely associated in the act of accommodation.

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[10] R. Granit, Über eine Hemmung der Zapfenfunktion durch Stäbchenerregung beim Bewegungsnachbild, *Zsch. f. Sinnesphysiol.*, 58, 1927, 95-110.

[11] Granit, On inhibition in the after-effect of seen movement, *Brit. J. Psychol.*, 19, 1928, 147-157.

[12] F. Fuchs, Experimentelle Studien über das Bewegungsnachbild, *Zsch. f. Psychol.*, 106, 1928, 267-315.

[13] Müller, *op. cit.*

[14] F. Leiri, Über optokinetische hervorgerufene visuelle Täuschungen, *Gr. Arch. f. Ophth.*, 119, 1928, 719-732; Gibt es einen extralentikularen Akkommodationsmechanismus in Auge?, *ibid.*, 121, 1928, 213-224.

In an extensive treatise Durup[15] has surveyed the more important experimental findings in the light of theories proposed to explain them, reiterating Hunter's view that independent explanations must be given for the various forms of the phenomenon. The author confirms previous descriptions of the Plateau spiral effect under the usual conditions of observation in which fixation is held on the center of the disk. This author's report of the transfer of this effect to the unstimulated eye with particular stimulus-distances adds one more to the many conflicting results on this problem. A neuro-muscular factor is suggested to explain the central processes, and retinal processes to explain the peripheral phenomenon.

Grindley[16] refutes Granit's claim that the inhibitory relation between the rods and cones gives rise to the movement-after-effect. He reasons that since red stimulates chiefly cones and blue chiefly rods, color should then affect the after-effect as measured by its duration. Each of several colors was used as stimulus (1) for a rod-free area and (2) for a larger area. According to Granit the ratios of the durations in the two cases should be greatest for red, but the differences were negligible, and Granit's results are interpreted as possibly due to the change in the perceptual configuration accompanying the change in the observation-distance.

An investigation by Sturm[17] upon the direction of the after-effect movement found its starting-point in Hunter's statement that under certain experimental conditions "it is the apparent direction of the real movement and not the actual direction which determines the after-movement."[18] Hunter had found that a series of oblique stripes placed across the opening in front of the movement gave a perception of the oblique movement during rotation and an opposite oblique after-movement. In order to test the 'purity' of the after-effect, *i.e.* its independence from the form of the projection-field, Sturm added three new control-situations, and four types of effect resulted: (1) reversal of the perceived movement, (2) opposite of the primary movement, (3) resultant of the two above, and (4) alternate. Two principal perceptual factors are proposed to explain these facts: (1) the "field" factor which functions when the presence or absence of the oblique grating in the projection-field affects the effect, and (2) the "reversal" factor which explains results affected by a grating during movement.

Method and Procedure

The present investigation was undertaken in an effort to study further some aspects of the observed after-effects of spiral movement. In order experimentally to answer certain questions a new form of presentation was chosen for detailed study. The stimulus was two identical spiral disks rotating at a known speed in opposite directions. These spirals were so arranged that they could be viewed together or singly by one eye, or together

[15] G. Durup, Le problème des impressions de mouvement consecutives d'ordre visuel, *Année psychol.*, 29, 1928, 1-56.

[16] G. C. Grindley, The relation between the rod and cone mechanisms in the after-effect of seen movement, *J. Physiol.*, 69, 1930, 53-59.

[17] M. Sturm, A study of the direction in the movement-after-image, *Psychol. Forsch.*, 14, 1931, 269-293.

[18] W. S. Hunter, Retinal factors in visual after-movement, *Psychol. Rev.*, 22, 1915, 257.

or singly by both eyes. A more detailed description of the apparatus and procedure follows.

Apparatus. Two white disks, 8 in. diam., upon which were drawn identical black spiral stripes, were mounted upon a two-spindle reducing apparatus and rotated by a motor. The centers of the two disks were approximately 11 in. apart. A black cardboard screen, placed behind the disks, concealed the apparatus and served as background for the disks. The disks themselves were the projection-ground for the after-effect, as this situation necessitated no shift in fixation.

O viewed the disks through small cardboard tubes from a distance of approximately 5 ft. These tubes prevented the two monocular fields of vision from overlapping. They were held at the level of *O*'s eyes by metal clamps and supporting rods. *O* then maintained his gaze halfway between the two disks, both during the objective movement and after its cessation.

A second set-up, adopted during the course of the experiment, simplified fixation but destroyed the two mutually exclusive visual fields which the first set-up had provided. The tubes were discarded and in their place a large rectangular box, extending from *O* to the disks, was used. The two ends of the box were open. Its dimensions were such that at one end it coincided with the cardboard background to which it was attached, while at the other end sat *O*. The sides were made of uniform black cloth, of a dull finish to prevent reflection from the 50-watt lamp which was clamped in a reflector to the inside of *O*'s end of the box. Two small cardboard screens (11 x 18 in.) were hinged to the inside of the ceiling of the box and could be lowered to a vertical position to conceal the disks. A single disk could thus be presented as stimulus. A tiny white cardboard cross, pasted on the center of the black background, served as a fixation-point.

A suitable stopping device consisted of a single lever so arranged that its operation both broke the electric circuit and arrested instantly the rotation of the disks.

The experiment was conducted in a dark room and light was furnished by a single 50-watt lamp suspended from the ceiling directly behind *O*.

The 90 *Os* were instructors in psychology and men and women graduate and undergraduate students in Brown University, all of whom were taking work in the department. All whose records were used in the study were naïve with respect to the problem under consideration.

Procedure. The experiment consisted of three parts. The first two were chiefly for purposes of control and comparison with the third. Common to all three situations were the identical fixation-point and the resulting use of indirect vision exclusively. These conditions made possible a direct comparison of the after-effects as they occurred in the three parts respectively.

In Part I of the experiment *O* was to view the right disk in clockwise rotation, monocularly, with the right eye. He did so by fixating the 'center point' with that eye, keeping the left eye closed. Similarly in Part II he was to view the left disk in counter-clockwise rotation, with the left eye. In both experiments a single disk was observed by a single eye. In Part III the two disks were viewed simultaneously by the two eyes respectively. The central fixation-point was held as before and the disks were rotated in their respective directions.

O viewed the rotating disk or disks for a period of 45 sec. He was instructed to report as accurately and completely as possible what he saw with respect to the

disk during that time. Upon cessation of the rotary movement he was to maintain his fixation and continue to describe what he saw until all was normal, *i.e.*, as it had been before the disks were rotated. Verbatim reports were made of *O*'s observations during and immediately after the trial.

The speed and duration of the rotary movement were held constant throughout the experiment. The speed (62 rotations of the disk per min.) and the 45-sec. duration were fixed upon after preliminary observations which showed them to be most favorable to the production of a definite after-effect. The duration was selected also with a view to preventing undue fatigue of the eyes. A stop-watch was used for timing the movement.

Results

The following results of the experiment are based upon the individual verbatim reports referred to in the above section.

The results from the first two experimental situations, in which a single disk was viewed indirectly by a single eye, extend to indirect vision the findings of previous investigators, who had employed fixation on the center

TABLE I

Number and Percentages of Observers Obtaining After-Effect

The figures are numbers and percentages of *O*s who served in the given experimental set-up. R = situation of clockwise rotation for right eye only. L = situation of counterclockwise rotation for left eye only. RL = situation combining R and L. A = set-up A (53 *O*s). B = set-up B (37 *O*s).

	Excluding second trials						Including second trials					
	R	R%	L	L%	RL	RL%	R	R%	L	L%	RL	RL%
A	34	64.2	27	50.9	39	73.6	37	69	32	60	44	83
B	35	94.6	33	89.2	36	94.6	37	100	36	98	37	100
Av.		79.4		70.0		84.1		85		79		91.5

of the disk itself. Thus, observation of the right disk produced essentially an experience of withdrawal and shrinking of spiral coils, which in the after-effect changed to an approach and expansion of these coils. In corresponding manner the left disk, rotated in the opposite direction, gave an appearance of approach during movement and withdrawal in the after-effect. It may be seen from Table I that 79.4% of the *O*s reported an after-effect for the right-eye-right-disk-situation, and 70% for the left-eye-left-disk-situation. Second trials, given to *O*s who had failed to obtain the after-effect, raised these results to 85% and 79% respectively. (See Table I.)

It seems from the present study, however, that the movement experience, both during rotation and in the after-effect, is more complex than has previously been supposed. Rather than a simple movement and its reversal, there really occurred movement experiences which varied greatly among *O*s and between different reports of the same *O*. Four types of movement

were reported which, although they usually occurred in combination, were nevertheless independent and isolable. A few excerpts from individual reports will serve to illustrate these various types:

(1) The spiral rotated to the right or left, according to the direction of rotation.

"The lines go around to the right."

(2) The entire disk, or the spiral lines within the disk, seemed to shrink (or expand).

"It seems to be growing larger now."
"The disk stopped dead still; suddenly got smaller and smaller."

(3) The disk was seen to move away from (or toward) *O*.

"It is coming way out (toward me); it is going to hit me; it comes nearer and nearer and nearer."
"The disk seems to come right up toward my eye."

(4) The spiral lines moved toward the center (or periphery) of the disk.

"Opposite effect to that of the other eye; the spirals are going in toward the center."
"The circles went in toward the center of the disk."

TABLE II

FREQUENCY OF OCCURRENCE OF THE FOUR TYPES OF MOVEMENT DURING AND AFTER ROTATION

The figures are percentages of *O*s obtaining an after-effect. R = situation of clockwise rotation for right eye only. L = situation of counter-clockwise rotation for left eye only. RL = situation combining R and L. m = perceived movement. ae = perceived after-effect.

	Rm	Rae	Lm	Lae	RLm	RLae	Total m	Total ae
Rotary	47.4	14.5	41.0	16.8	35.8	5.3	41.3	11.3
Expansion	14.6	31.7	29.8	15.3	8.4	8.2	17.6	16.8
Approach	59.2	85.4	53.6	78.8	66.6	93.3	59.1	85.8
Toward periph.	51.3	17.3	51.5	12.0	21.6	5.4	11.5	11.7

The report of a single one of these types of movement was taken as the criterion of the existence of the after-effect, a fact which should be remembered when consulting the tables. The relative frequency with which the four types of movement were reported, during movement and in the after-effect, may be seen from Table II. It will be noted that the withdrawal and approach movements of the disk were most frequently noted, and may be considered as particularly characteristic of the after-effect. Eighty-six percentage of the *O*s who obtained an after-effect reported this type of movement in contrast to the 11%, 16%, and 11% who reported the other three types.

The interesting results of the study from a theoretical standpoint were

obtained, however, from Part III, in which the two quite distinct and independent visual fields of the two eyes were combined into what we may call a monocular-binocular situation, in Set-up A. If the normal after-effects of the two eyes continued to occur under these conditions, the fact should have important consequences for a theoretical explanation of the phenomenon.

It was found that during rotation each of the two disks retained its characteristic types of movement, and that at the cessation of the rotation the usual after-effects resulted simultaneously. It may be seen from Table I that 74% of the *O*s obtained this double after-effect and that this figure was raised to 83% by adding second trials. The following excerpts from individual reports illustrate the nature of this after-effect:

"The right is coming toward me, and the left is going away."
"I have a strong sensation of the right coming toward me and the left fading away; it lasted longer with the two eyes."
"The right disk is coming out of the distance and the left is going off into the distance."

This result held also in Set-up B, in which situation the two visual fields overlapped and both disks were viewed by both eyes simultaneously. Ninety-five percentage of the *O*s obtained this after-effect, or 100% if second trials are considered.

The number of *O*s who obtained this double after-effect, as presented in Table I, suggests that the contrast of movements in this situation heightened the likelihood of an after-effect. The after-effect was obtained by a larger proportion of *O*s in this situation than in either of the two monocular situations. Moreover, certain *O*s obtained this effect although they had failed to obtain either of the monocular effects. It is true, however, that practice in observation and report may influence this finding. The influence of the two visual fields upon each other appears in the *O*s' descriptions, since such reports were made as: "The two disks move in opposite directions," and "The right disk comes toward me, but the left goes away." In this situation as well as the first two parts, the tables show that a greater proportion obtained the after-effect under the conditions of Set-up B than under those of Set-up A. This is probably explained by the greater ease of fixation in this situation.

The after-effect has been traditionally known as an opposite or negative after-image of movement. Hunter considered certain of his experimental data according as they showed an "s.-a.-m." or after-movement in the same direction as the primary movement, or an "op.-a.-m." or after-movement in the opposite direction to that of the primary movement. Such reversals were also noted in this study. Certain *O*s reported: "I get the opposite

effect." A comparison, however, of the number of types of movement reported during rotation and in the after-effect suggests that the after-effect is more than a simple contrast phenomenon. Whereas during rotation a combination of types of movement usually occurred, the after-effect was more frequently dominated by a single movement, the approach or withdrawal of the disk. This fact is brought out to some extent in Table II which indicates, for example, that in the RLae-situation 93% of the *O*s reported approach and withdrawal, while only 5–8% reported the other three types of movement. During the RLm-situation less importance could be given to a single type of movement. The RL-situation shows this fact most clearly. In this case the after-affect nearly always consisted of the single type of movement, the approach and withdrawal of the disk.

The introspective report of the nature of the after-effect has already been shown to vary with the individual. Depth effects, for example, were observed by some *O*s and not by others. Rough estimations on duration and intensity also showed great individual differences. Duration varied from a momentary perseveration of the experience of rotation to an after-effect of over a minute. The intensity varied from an almost imperceptible change of movement to a sudden and striking effect which completely surprised the *O*.

A small number of supplementary observations were made in which a slight modification of Set-up B permitted both disks to be viewed in simultaneous rotation by a single eye. Under these conditions all *O*s obtained an after-effect similar to that obtained in Part III of the main experiment. The opposition of the two movements was as marked in this case as when both eyes were used. These results show that the normal after-effect results when two regions of a single retina are stimulated by spatially separated and oppositely rotating spirals.

Summary

(1) An after-effect of movement was demonstrated on a rotating spiral disk under conditions of monocular and indirect vision.

(2) Four types of movement were variously reported, any one of which served as criterion for existence of this after-effect. These may be characterized as (1) rotary movement, (2) expansion and contraction, (3) approach and withdrawal, and (4) movement of the spiral lines toward the periphery or center of the disk.

(3) The after-movement was predominantly a movement toward or away from the *O* (type 3 above), depending upon the direction of rotation of the disk observed. The after-affect most frequently consisted of this single

type of movement in comparison to the experience of rotation where two or more types of movement were usually observed.

(4) Simultaneous observation by the two eyes respectively of two identical spiral disks, spatially separated and rotating in opposite directions, was followed by an after-effect of the opposing movements of the two disks, one toward and one away from the observer. This after-effect occurred in certain instances in spite of the *O*'s previous failure to obtain both, or even a single, monocular after-effect.

(5) This binocular after-effect occurred also when the two visual fields were allowed to overlap.

(6) Individual differences were noted with respect to the intensity and duration of the after-effect, and its introspective nature.

(7) Simultaneous observation by a single eye of the two rotating spiral disks was followed by the after-effect observed in Part III of the main experiment.

Theoretical Discussion and Conclusions

Many theories have been formulated to explain the movement-after-effect. Critical reviews of these theories appear in the writings of Wohlgemuth, Hunter, and Durup, and need not be included here. The present writer will attempt simply to evaluate those theories which have either been shown inadequate, or have been supported and strengthened by the results of the present investigation, and on this experimental basis suggest what seems to be the most satisfactory explanation of the spiral illusion.

Previous theories may be grouped as (1) Central Theories, including those which hold the phenomenon to be due primarily to processes within the central nervous system, and (2) Peripheral Theories, which hold the phenomenon to be predominantly retinal or muscular in origin.

Hunter's explanation in terms of retinal activity was formulated principally on the basis of the author's inability to demonstrate the transfer of the after-effect to the unstimulated eye, and the occurrence of a normal after-effect when two opposite movements viewed monocularly were superimposed. Certain facts, however, seem to render such a theory inadequate for the spiral phenomenon. In the first place, Ehrenstein and Durup have since demonstrated the denied transfer.[19] Secondly, the many possible types of movement shown by the present study to characterize the after-effect virtual-

[19] W. Ehrenstein, Versuche über die Beziehungen zwischen Bewegungs- und Gestaltwahrnehmungen, *Zsch. f. Psychol.*, 96, 1925, 305-352; G. Durup, Le problème des impressions de mouvement consecutives d'ordre visuel, *Année psychol.*, 29, 1928, 1-56.

ly turn the theory into a central theory, since in its present form it fails to explain, except by an associative factor, how the fading retinal after-images may be interpreted now as movement away from *O*, and now as shrinking of the disk. However, such a simple central view is apparently not in accord with the facts early demonstrated by Bowditch and Hall[20] and since confirmed by most investigators that the phenomenon is not only striking to the *O*, but occurs in spite of the distraction of his attention from the stimulus movement.

From the results of the present study it seems that a more satisfactory explanation of the spiral phenomenon is to be found in the activity of the visual musculature during and immediately after observation of the rotary movement. Of muscular theories previously formulated those based upon the coördinate action of the extrinsic eye muscles have long been adversely criticized because they fail to explain the basic fact of the perception of radial movement, which movement demands that coördinate movements occur in all directions simultaneously.

This difficulty has been recognized by Leiri who, therefore, bases his explanation on the delicate processes of the accommodation mechanism rather than on changes in the larger musculature of the external eye muscles. But Leiri, too, fails to explain the two simultaneous opposite after-effects of the two eyes which the present experiment demonstrates, since his "voluntary accommodation innervation" involves a close coördination between the activity of the two eyes. He seems not to have been acquainted with Hunter's demonstration of the after-effect when two oppositely rotating spirals were superimposed, for such a finding involves definitely the independent activity of the two eyes.

In this paper it may be very tentatively suggested that the mechanism by which the illusion occurs seems in some respects to be basically of the nature of the effector events posited, upon the basis of experimental work in various psychophysiological fields, by such writers as Münsterberg, Delabarre, Washburn, Holt, and Langfeld. As Langfeld clearly says in discussing the general nature of perception and motor phenomena, perception takes place only upon the occurrence of some motor response "incipient" if not overt. These responses, while not ordinarily a part of direct awareness, as such, are nevertheless according to this theory indispensable antecedent events for every perceptual experience. In the narrower field of visual perception Langfeld answers the criticism raised

[20] H. P. Bowditch and G. S. Hall, Optical illusions of motion, *J. Physiol.*, 3, 1881, 296-301.

against eye-movements as an explanation of perceptual illusions, by pointing out that such a theory as he holds "does not call for actual movement, but merely a specific change in the efferent system."[21] It is thus possible, he continues, "that a change in muscle tonus produced by the antagonism of the muscles involved is a physiological cause of such phenomena."[22] Langfeld suggests further that the motor responses bringing out the phenomenon need not be restricted to the muscles of the eye, but, by a process of conditioning, may take place in any muscles of the body.

Returning to the spiral phenomenon, we see how such a view suggests an explanation of the experimental findings of this study. During the observation of the objective movement the continued stimulation of the eyes by the rotation of the disks sets up certain very definite changes in the tonus of the eye-muscles. These changes probably occur both in the external muscles, and, more especially, in the internal muscular systems of the mechanisms of accommodation and convergence. Upon cessation of the movement (in this experiment after 45 sec.), these well established changes are completely upset. The stimulus-situation which created them is suddenly removed, and the return of the muscles involved to their normal state at once begins. Those muscles in which an increase in tonus had occurred now gradually relax, those in which the tonus was correspondingly decreased now return to their former state of greater tonicity. This adjustment, of course, involves activity of the central nervous system. The result perceptually is an experience, only part of which is determined by activities initiated in the retina. During objective movement the changes reported might be those accompanying apparent decrease in size and withdrawal and then, for example, the return in certain instances seemed like a complete reversal, a swelling out toward the *O*, while in certain other instances the return to balance led to other introspectively reported phenomena.

The simultaneous occurrence of the two opposite after-effects in the two eyes is thus seen as possibly taking place because the two eyes are presented with different stimulus-situations. The tonus of the muscles under these conditions is affected independently of the muscles of the other eye and of the other muscles in the same eye, and the return of the two sets of muscles to normal thus gives rise to the two opposite experiences. In the light of this view it is not surprising that occasionally the simultaneous occurrence of the two opposite muscular events is accom-

[21] H. S. Langfeld, A response interpretation of consciousness, *Psychol. Rev.*, 38, 1931, 101.
[22] *Loc. cit.*

panied by a strong opposition in the binocular after-effect, even when the changes of tonus in the individual eye had been too slight to produce the monocular after-effect.

In the same manner as described above it is possible for a single eye, stimulated by two different stimulus-movements, which affect respectively the tonicity of different muscles, to perceive simultaneously two opposite experiences accompanying the return to normal of the two patterns of tonic activity. This view accounts for the supplementary experimental finding that either eye, observing both disks, obtained an after-effect of both disks moving in opposite directions.

The theory accounts also for a previous experimental finding, namely that the form of the after-effect depends to some extent upon the ground on which it is projected. The phenomenon is not so much one of a definite movement as it is one of expansion or contraction of the visual field. Therefore, whatever stimulus-object is presented to the 'unbalanced' eye may cause a definite experience, in part dependent upon the after-effect in the optic mechanism and in part due to the present stimulus-situation.

In the view of the present writer, therefore, it may be said that a motor theory of the sort outlined above is adequate to explain the facts determined in the present experimental study of the after-effects of visually observed movement.

COMPARATIVE ACCURACY IN THE LOCALIZATION OF CUTANEOUS PRESSURE AND PAIN

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Recent attempts which have been made to determine the comparative accuracy of cutaneous localizations of pressure and of pain have resulted in an apparent deadlock. On the one hand, Ponzo[1] published experimental results which indicate that the average error of localization for pain is slightly smaller than that for pressure. In connection with this finding, he pointed out that it is biologically more essential that an organism be able to localize accurately injurious than non-injurious stimulations. On the other hand, Von Skramlik[2] and Mayer[3] have more recently presented experimental results which are inconsistent with those of Ponzo. According to their reports, stimulations of pain were localized with a lower degree of accuracy than were stimulations of pressure. Although the tables of Ponzo revealed only a small degree of difference in the errors of localization for pain and pressure, the results of Von Skramlik and Mayer indicated that the error for pain is considerably larger than that for pressure.

The controversial discussion which followed the appearance of these publications was intended to provide an explanation for this contradiction in experimental findings. Several points of difference, such as difference in method and in mode of stimulation have been specified as possible sources for this inconsistency in experimental results. Ponzo used the method to which the name of Weber is attached. The *O* kept his eyes closed throughout the trial and localized the point of stimulation by means of a pointed instrument. He was permitted to move the instrument about on the skin until he was satisfied with the localization. Von Skramlik and Mayer, however, used the method of Volkmann, which required the *O* to open the eyes after the stimulation in order that the localization might be made with the aid of vision.

In order to determine whether the contradiction in these experimental results

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[1] M. Ponzo, Recherches sur la localisation des sensations tactiles et dolorifiques, *Arch. ital. de biol.*, 55, 1911, 1.

[2] E. von Skramlik, Lokalisation der Empfindungen bei den niederen Sinnen, *Zsch. f. Sinnesphysiol.*, 56, 1925, 135 ff.

[3] B. Mayer, Beitrag zur Lokalisation von Schmerzempfindungen, *ibid.*, 56, 1925, 141-153.

actually was a function of difference in method, Mayer[4] performed additional experiments in which localizations of pain and of pressure were made by both of the methods just mentioned. Her results are in both cases in harmony with her original finding. The average error for pain localizations was larger in both cases, and in about the same degree in each case. She concluded that difference in method cannot be designated as the foundation for this conflict in results.

In all of the studies mentioned above, a hair mounted in a Von Frey esthesiometer was used in the stimulation of pressure. For the arousal of pain, Ponzo used a sharp metal needle, but Von Skramlik and Mayer used a thistle (*Stacheln von Carduus acanthoides*). Von Skramlik[5] asserted that a needle does not consistently arouse pure pain but a complex of pain and pressure sensations, and Mayer[6] cited Von Frey as authority for ruling out the needle as the instrument for the stimulation of pain. In her own work, Mayer found that the needle aroused pure pain in less than 20% of the stimulations. It is her belief, therefore, that Ponzo failed to excite pure pain, and that his *O*s were called upon to localize, for the most part, pressure-pain complexes, which Ponzo took to be pain. The controversy is thus reduced to a matter of applying the term pain to essentially different cutaneous qualities. In order to test this hypothesis, Mayer, in the same study in which she tested the possible influence of difference in method, required her *O*s (Dr. Strughold and herself) to decide into which of three classes of sensation each of the stimulations by the thistle (*Stachelborste*) belonged—(1) pure pressure, (2) pure pain, or (3) pressure-pain complex. Calculation of the average error for each of these classes revealed that the error of localization was largest for pain and smallest for pressure-pain complex. The average error for pure pressure was slightly larger than that for pressure-pain complex, but decidedly smaller than that for pure pain.

In view of these additional determinations, Mayer and Von Skramlik are apparently convinced that the conflict is really resolved, and that they have established the fact that the average error of localization is greater for pain than for pressure. However, it would seem desirable that these experiments be repeated, particularly in view of the fact that in her second study Mayer had only two *O*s, and that in the controversy with which we are here concerned Von Skramlik merely came forward with a brief polemical paper.

Experiment I

As a first step in the direction of making a contribution toward the clarification of certain points underlying this controversy, we made localization trials under conditions essentially like those in the last mentioned work of Mayer. A horse-hair, mounted in a Von Frey esthesiometer, was used in the stimulation of pressure. The hair had a tension strength of 1 gr./mm. Since the thistle which Mayer used in the stimulation of pain (*Stacheln von Carduus acanthoides*) was unavailable, we used instead a spine from the purple top thistle (*Carduus lanceolatus L*) which is

[4] Mayer, Über die Ortsbestimmung von reinen Schmerzempfindungen, *ibid.*, 58, 1927, 32-37.
[5] Von Skramlik, Zur Frage der Lokalisation von Schmerzempfindungen, *ibid.*, 58, 1927, 28-31.
[6] Mayer, *op. cit.*, 1927, 34.

commonly found in the eastern part of the United States. In her earlier work, Mayer also used a different thistle (*Onopordon bracteatum*).[7]

The stimulations were made on the back of the hand. Before making the trials, 10 ink spots were placed on the back of each hand at the points at which *E* expected to stimulate. The ink spots were placed neither over blood vessels nor at other points of prominence. In a first series of trials, the 10 ink spots were placed rather close together, within an area of about a square inch just posterior to the union of the index and middle fingers. We made series of ten trials alternately on the two hands, thus providing between series a period for partial recovery from fatigue. We alternated the kind of stimulation which was used at the beginning of successive sittings. At odd sittings, the esthesiometer was used in the first series of trials, and at even sittings the thistle was first used. There was regular alternation of type of stimulation after every 10 trials within the sitting. Localization was made with the aid of a pointed pencil. *O* was permitted to move the pencil to and fro on the skin until he had localized as accurately as possible. The eyes were kept closed throughout stimulation and localization periods. The localizing hand, provided with the pencil, was kept at a designated spot on the table, 15 in. from the hand upon which stimulation was to be made. The localizing hand was not moved from the resting position until the stimulating instrument had been withdrawn by *E*. In order to keep kinesthesis, in general, as nearly constant as possible, the *O* was required to keep both feet on the floor throughout the sitting.

Two of the 8 *O*s were members of the staff in psychology, and the other 6 were undergraduate majors in the department. A period of training in localization preceded the experiment proper.

With regard to the comparative adequacy of the two stimuli in arousing the specific qualities with which we are here concerned, it is clear that the hair is more satisfactory in that the stimulations made by it never resulted in complications with the other quality. These stimulations always produced simple pressure only. The thistle, however, frequently produced a pressure-pain complex—an experience in which the qualities of pressure and pain were combined. As already noted, the needle has been found to be unsatisfactory for the arousal of pain because it frequently arouses pressure. Von Frey designated the thistle as more satisfactory than the needle on the ground that in most of these stimulations no pressure was observed. Although the pressure component is greatly reduced by the use of the thistle, nevertheless our *O*s reported that usually a faint fine contact was experienced just a moment before the quality of pain appeared. However, when the pain quality appeared, it tended to mask the pressure component.

Moreover, the thistle did not at all times arouse a quality of pain of the same character. Certain pains were sharper, more lively and more stinging in character than others. Sometimes there was a suddenness in the manner in which the sensation of pain emerged to a full quality of clear pain; at other times, a number of graded changes were perceived as the quality became clearer and brighter. There was distinctly less variability in the character of the pressure qualities than in those of the pain qualities. With regard to those stimulations which resulted in pressure-pain complexes, the *O*s reported that as a general rule the two qualities were clearly dis-

[7] Mayer, *op. cit.*, 1925, 143.

tinguishable. Frequently an *O* reported incipient pain, specifying that this element was the brighter component in a pressure-pain complex. At other times, pain was reported as occurring along with a dull, dragging pressure. There were only a few instances in which the quality of pain and that of pressure were so intermingled that the *O* could not clearly distinguish between the two qualities in the complex.

The average errors of localization for both kinds of stimulation—pressure and pain—are presented in Table I. The averages were determined from 50 trials with each stimulus on each hand.

The table shows that, for the most part, the average error of localization for pain is smaller than that for pressure. There are exceptions for 2 *O*s on the right hand and for a single *O* on the left hand. Thus, in 13 of the 16 instances, the average error for pain is smaller than that for pressure.

TABLE I

SHOWING THE AVERAGE ERROR OF LOCALIZATION IN MM. FOR PRESSURE STIMULATION BY THE HAIR OF A VON FREY ESTHESIOMETER AND FOR PAIN STIMULATION BY THE THISTLE ON THE BACK OF THE HAND, WHEN THE POINTS OF STIMULATION ARE CLOSE TOGETHER

	Left Hand		Right hand		Both hands combined	
O	Pressure	Pain	Pressure	Pain	Pressure	Pain
Ba	4.30	4.66	4.24	2.98	4.27	3.82
Be	7.10	4.73	3.63	3.10	5.36	3.91
Br	3.28	3.15	4.40	3.71	3.84	3.43
J	6.03	5.36	7.63	5.80	6.83	5.58
M	3.46	3.23	2.86	2.56	3.16	2.89
P	4.90	3.00	4.26	3.23	4.58	3.11
W	5.13	4.00	3.96	4.93	4.54	4.46
Z	3.66	3.30	3.13	3.35	3.39	3.32
Av.	4.73	3.92	4.26	3.70	4.49	3.81

When the averages of the two hands of each *O* are combined, the average error for pain is smaller in every case. Although the difference between the averages for pressure and for pain is small in a number of cases, it is nevertheless apparent that pain tends to be localized slightly more accurately than pressure.

EXPERIMENT II

In order to determine whether, in the foregoing work, the fact that the 10 ink spots were close together had any influence upon these results, we made a series of trials in which the 10 spots were more widely scattered over the back of the hand. It seemed possible that, in this experiment, the *O* may have developed the habit of localizing in a restricted area, and that differences in the magnitude of the two errors may have been minimized in consequence of the fact that the localizing instrument was placed regularly in the same limited field. Table II embodies the aver-

ages for the localizations with ink spots more scattered. Each average in this table was computed from 100 trials.

The table reveals that the average error of localization for pain is smaller than that for pressure at every point of comparison. Moreover, the difference, although again not great, is a matter of greater comparative significance. It is evident that the scattered arrangement of points of stimulation is more favorable to the demonstration of relative accuracy of localization of these two qualities.

TABLE II

SHOWING THE AVERAGE ERROR OF LOCALIZATION IN MM. FOR PRESSURE STIMULATION BY THE HAIR OF A VON FREY ESTHESIOMETER AND FOR PAIN STIMULATION BY THE THISTLE ON THE BACK OF THE HAND, WHEN THE POINTS OF STIMULATION ARE SCATTERED

	Left hand		Right hand		Both hands combined	
O	Pressure	Pain	Pressure	Pain	Pressure	Pain
Ba	5.44	3.94	3.90	3.20	4.67	3.57
Be	4.51	3.58	5.10	3.62	4.80	3.60
Br	4.75	3.08	4.79	3.41	4.77	3.24
J	4.26	3.00	4.42	3.24	4.34	3.12
M	3.80	3.58	3.90	3.48	3.85	3.53
P	4.04	3.10	4.58	3.60	4.31	3.35
W	5.20	4.22	6.72	4.17	5.96	4.19
Z	4.80	3.17	4.87	2.74	4.83	2.95
Av.	4.60	3.45	4.78	3.43	4.69	3.44

With regard to the comparative accuracy of localization on the right and left hands, we note that the *O*s tend to confirm Dallenbach's recent finding[8] that the difference is essentially negligible, and his postulate that one may safely predict the magnitude of the error of localization on one hand in terms of the results of trials made on the other. The slight discrepancies which appear in the table between localizations on the right and left hands of an *O* are distributed, indicating that certain *O*s were slightly more accurate in localizations on the right and others on the left hand.

EXPERIMENT III

In the foregoing experiments we followed the traditional procedure of using the Von Frey esthesiometer in the stimulation of pressure. For the stimulation of pain we used a thistle. In the present experiment we endeavored to determine in some degree the extent to which pressure sensations enter into the experiences which arise when stimulations are made by

[8] K. M. Dallenbach, A comparative study of the error of localization on the finger-tips, this JOURNAL, 44, 1932, 327-331.

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the thistle, and whether the pressure component in such stimulations differs with variation in intensity of application of the thistle.

A series of 10 stimulations were made on each hand with heavy or maximal application of the thistle (application in which the thistle punctured the skin), then 10 light stimulations were made in such a manner that the thistle point made only a slight indentation on the skin surface. Experimentation continued until 250 localizations at each of these two intensities had been made. The error of localization was measured in the same manner as in the previous work. Again, stimulations were made alternately on the two hands in order to provide periods for partial recovery from fatigue. At successive sittings as well as in successive series in the same sitting, we alternated the type of stimulation. If heavy stimulation was used first in a given sitting, light stimulation was used first at the next sitting. After each trial the *O* was required to specify into which of the following three classes the quality of sensation belonged: (1) simple pressure, (2) pressure-pain complex, and (3) simple pain.

TABLE III

SHOWING THE NUMBER OF THE THREE CUTANEOUS QUALITIES (PRESSURE, PRESSURE-PAIN COMPLEX AND PAIN) WHICH WERE REPORTED IN 250 LIGHT AND 250 HEAVY STIMULATIONS WITH THE THISTLE ON THE BACK OF EACH HAND

	Light stimulation						Heavy stimulation					
O	Left hand			Right hand			Left hand			Right hand		
	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.
Ba	206	39	5	194	52	4	1	31	218	1	20	229
Be	214	27	9	215	27	8	8	5	237	12	6	232
Br	196	49	5	200	46	4	1	19	230	0	10	240
J	204	45	1	210	37	3	8	20	222	13	18	219
M	178	60	12	171	63	16	1	44	205	1	40	209
P	184	54	12	185	46	19	9	23	218	4	16	230
W	191	53	6	199	49	2	13	35	202	7	34	209
Z	212	34	4	215	26	9	7	54	189	6	57	187
Total	1585	361	54	1589	346	65	48	231	1721	44	201	1755

Table III reveals the manner in which the 250 light and 250 heavy stimulations on each hand were classified by each of the 8 *O*s. It will be noted that more than 80% of the heavy applications resulted in simple pain. Light stimulations produced simple pressure more frequently than any other quality, but in not quite so large a percentage of cases as that in which heavy stimulation produced simple pain. It is, accordingly, not safe to assume that a thistle will arouse the same quality of sensation every time it is applied to the skin, even when the stimulations are made at about the same intensity. Approximately 15 to 20% of the stimulations, varying somewhat from *O* to *O*, resulted in pressure-pain complex.

The average errors for the three qualities—simple pressure, simple pain and pressure-pain complex—are presented in Table IV. Turning first in that table to the combined averages of the 8 *O*s, it will be noted that the

average error for pressure-pain complex is smaller than that for either of the other qualities in three of the four points of comparison. To this extent we have confirmed the finding of Mayer, already referred to, that pressure-pain complexes are localized with a higher degree of accuracy than are either simple pain or simple pressure. However, in the fourth case in the combined averages, the average error for simple pain is the smallest. This result is positively inconsistent with the contentions of Mayer and Von Skramlik, who found the error for pain to be greatest of all. If we compare the average errors for the two qualities of simple pain and simple pressure, it will be seen that that for the former is less than that for the

TABLE IV

SHOWING THE AVERAGE ERRORS OF LOCALIZATION FOR THE THREE CUTANEOUS QUALITIES (PRESSURE, PRESSURE-PAIN COMPLEX AND PAIN) INTO WHICH 250 LIGHT AND 250 HEAVY STIMULATIONS WERE CLASSIFIED

	Light stimulation						Heavy stimulation					
O	Left hand			Right hand			Left hand			Right hand		
	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.
Ba	5.02	4.48	3.40	4.04	4.30	3.25	8.00	5.25	4.75	2.00	5.45	3.70
Be	4.50	4.03	3.33	4.80	3.59	3.50	5.00	3.80	3.96	3.91	3.50	4.01
Br	3.99	3.04	2.80	4.95	3.76	4.25	6.00	2.94	3.15	—	5.80	3.52
J	4.52	3.06	2.00	4.97	4.24	4.33	4.25	4.75	4.11	4.46	4.66	4.01
M	4.17	3.46	4.25	3.61	4.33	3.62	5.00	3.59	3.56	4.00	4.10	3.54
P	4.21	3.64	4.25	4.10	3.50	4.42	3.44	3.60	3.80	3.00	3.87	3.50
W	4.76	4.28	5.33	6.57	5.18	10.00	4.46	2.77	3.96	7.71	7.05	5.95
Z	3.95	3.97	6.00	3.71	2.38	2.44	5.42	3.12	3.11	5.83	2.92	3.13
Av.	4.39	3.74	3.92	4.59	3.91	4.47	5.19	3.72	3.80	4.41	4.66	3.92

latter in every case. This result also runs counter to the trend of the work of Mayer and Von Skramlik.

By comparing the average errors of the particular *O*, it will be noted that in eleven of the thirty-two points of comparison the average error for pressure is smaller than that for pain. To this extent this table contains results which are apparently out of harmony with the results in our other tables. However, Table IV needs to be studied in connection with Table III, which gives the number of trials upon which each average in Table IV is based. By referring to Table III, it will be found that wherever the error for pressure in Table IV is smaller than that for pain, the average of one or the other is determined from a very limited number of trials, sometimes from a single determination. The comparisons in these 11 cases are therefore not very reliable. In the other 21 points of comparison, however, the average error for pain is smaller than that for pressure. In these 21 points, the number of cases is sufficient, generally, for reliable comparison.

If cutaneous pain is more accurately localized than pressure, a larger number of perfect localizations, or localizations with zero error, might be expected to appear in the pain columns than in those for pressure. Accordingly, we determined the number of such localizations which fell to each of the three qualities. Table V shows the result of this enumeration.

Although there are as many or almost as many perfect localizations for pressure as for pain at several points of comparison for particular *O*s, nevertheless all of the *O*s taken together amassed a significantly greater number of localizations with no error in the pain than in the pressure stimulations. Since fewer perfect localizations fell in the pressure-pain complex columns, it may at first glance appear that this result is at odds with our finding that

TABLE V

SHOWING THE NUMBER OF PERFECT LOCALIZATIONS (NO ERROR) WHICH FELL TO EACH OF THE THREE CUTANEOUS QUALITIES (PRESSURE, PRESSURE-PAIN COMPLEX AND PAIN)

	Left hand			Right hand		
O	Pressure	Pressure-pain	Pain	Pressure	Pressure-pain	Pain
Ba	9	2	10	17	1	20
Be	15	2	25	11	5	24
Br	13	4	27	9	6	20
J	7	4	11	7	4	13
M	9	10	9	15	7	14
P	14	3	15	9	3	17
W	11	7	20	6	2	12
Z	29	12	31	24	11	30
Totals	107	44	148	98	39	150

these localizations are generally made with higher accuracy than those of pressure in particular, and in general than those of pain also. However, reference to Table III will show that in this experiment relatively fewer pressure-pain complexes were aroused than experiences of simple pressure and simple pain. Thus, the table of perfect localizations is composed from a relatively large number of pain and pressure stimulations and from comparatively few stimulations of pressure-pain complex. By comparing the totals in Tables III and V, it will be observed that the 8 *O*s made 205 perfect localizations for pressure in a total of 3266 trials, 83 perfect localizations for pressure-pain complex in 1139 stimulations, and 298 perfect localizations for pain in 3595 trials. Thus, there was one perfect localization in about every 15 in the case of pressure stimulations, one in about every 13 in the case of pressure-pain complex, and one in every 12 in the case of pain. Again, the results indicate that the quality of pain has an advantage as compared with pressure in regard to the matter of accuracy of localization.

If it is justifiable to infer that more perfect localizations should appear in the column of that quality which is most accurately localized, it would seem to follow that larger errors should tend to appear in the column of that quality which is least accurately localized. Accordingly, we have arranged a table which reveals the largest errors that fall to each of the three classes of qualities in the 250 light and 250 heavy stimulations by the thistle.

Table VI indicates that the largest error for the pressure-pain complex is almost invariably smaller than that either for simple pain or simple pressure. The comparison of largest errors between pressure and pain shows no significant difference, although, in general, that for pressure is slightly greater than that for pain. The evidence which this table brings forward tends

TABLE VI

SHOWING THE DISTRIBUTION OF LARGEST ERRORS OF LOCALIZATION, WITH THE NUMBER OF TIMES THE ERROR OCCURRED, IF MORE THAN ONCE, INDICATED IN PARENTHESES

	Left hand			Right hand		
O	Pressure	Pressure-pain	Pain	Pressure	Pressure-pain	Pain
Ba	14, 13 (4)	12, 11, 10	14, 12	12(2), 11, 10(4)	11(2), 10(2)	13, 11
Be	12(4), 11(4)	10(2)	11(3), 10(7)	13, 12(3)	10	11(5), 10
Br	14, 12(2)	10(3)	11. 10	13, 11(6)	10(2)	10(3), 9
J	13, 12(6), 11(4)	11, 10	12(4), 11(2)	14(2), 12(2), 11(7)	10	13, 12 11(3)
M	13, 12, 11(3)	12, 11, 10	12, 11(3), 10	13, 10	10(4)	11(2), 10(3)
P	13, 12, 11	12, 11, 10	13, 12, 11(3)	15, 10(5)	10	14, 12(2), 11
W	12, 10(9)	10(2)	14, 11, 10(5)	20, 15, 12(10), 11(10)	12(2), 10(7)	14(2), 12(9), 11(6)
Z	13(2), 12(2), 11	10(3), 8	12(2), 11, 10(2)	11(3), 10(8)	10, 9	10(6), 9(5)

to support, even if only to a slight degree, our previous findings with regard to the comparative accuracy of localizations of simple pain and simple pressure.

Our results seem to provide a partial substantiation for the positions of both of the parties to the controversy referred to at the outset. The work of Ponzo is confirmed in regard to the point that the error of localization for pain is smaller than that for pressure, and also in respect to the matter that the difference between the errors of localization for pain and for pressure is small. Our results tend to corroborate in a general way the finding of Mayer that the error of localization for pressure-pain complex is smaller than that for either pressure or pain.

THEORETICAL DISCUSSION

If one kind of cutaneous stimulation can be localized with greater accuracy than another, there must be some foundation for any difference which occurs in this respect. Ponzo, as we have already pointed out, explained his discovery that the error of localization for pain was smaller than

that for pressure in connection with the argument of *biological necessity*, that is to say, it is more essential to the well-being and survival of an organism that pain be accurately localized than pressure. *Practice* cannot be alleged as a principle of explanation here because we have much more practice in localizing pressure than in localizing pain.

If a complete picture were available in regard to the events which take place in the nervous system in these three kinds of stimulation, a satisfactory explanation of the differences in respect to accuracy of localization might be given. However, until all of these facts are determined, we shall be required to infer the probable basis for the difference in terms of the few isolated facts which are at our disposal.

Obviously the after-sensation which is experienced after the removal of the stimulus and during the period of localization is the chief guide to localization in stimulations of touch. The after-sensation may be said to represent a continuation of stimulation after the removal of the stimulus. Since localization is made chiefly in terms of the after-sensation, and since there is a difference in the accuracy of localization of the three qualities which the thistle aroused, it would seem to be probable that the composition of the after-sensations and the nature of the conditions of stimulation underlying them must be different in the three cases.

One or two recent findings point to the probability that there is a difference in the conditions of stimulation in these three qualities. Piéron[9] has shown that the rate of propagation of nerve impulses in a cutaneous nerve varies with the kind of stimulation. Erlanger, Gasser, and Bishop[10] have shown that during stimulation a nerve bears impulses travelling at different rates of speed. Now pressure-pain complex results from a combination of two kinds of stimulation—pain and pressure. The cutaneous nerve thus bears impulses which travel at different rates in such a stimulation, and the specific impulses for pain and for pressure may also differ in other ways which are as yet undetermined. The mode of integration of these impulses in the cortex would seem to be essentially of a different order than that which operates in the integration of a volley of impulses which are relatively homogeneous in respect to rate of propagation and other characters. It is clear that the greater accuracy of localization in pressure-pain complex may find its explanation in the difference in type of cortical integration.

[9] H. Piéron, Nouvelles données sur la dissociation des douleurs cutanées, *J. de Psychol.*, 28, 1931, 279-282.

[10] J. Erlanger, H. S. Gasser, and G. H. Bishop, The compound nature of the action current of nerve as disclosed by the cathode ray oscillograph, *Amer. J. Physiol.*, 70, 1924, 624-666.

The assumption that the integration of more diverse kinds of impulses provides the basis for improved accuracy in localization is not so adequate in explaining the difference in accuracy of localization in the two simple qualities of pressure and pain. In both of these cases, supposedly a homogeneous volley of impulses pass over the nerve during stimulation. However, it is possible that there is greater variability of impulse rates around the modal rate or a greater difference in the character of the impulses in the case of pain than in that of pressure. If it should eventually be determined that this is the case, the assumption that greater accuracy of localization is correlated with greater diversity of the body of impulses which are being integrated would serve to explain the difference in accuracy of localization in pain and pressure stimulations also.

Investigators who are concerned with problems which find their setting in the field of touch are called upon to relate the results of their experimental efforts to one or the other of two theoretical hypotheses, which have been labelled the *Qualitative* and *Quantitative* theories of touch. This is not the place or time to evaluate the relative merits of these two theories, but inasmuch as neither of them has completely displaced the other, it would seem to be proper to allocate to each theory those facts which seem most clearly to fall to it.

Those who adhere to the qualitative theory contend that pain and pressure arise from the stimulation of a different kind of receptor. Defenders of the quantitative theory apparently take the position that pain results from maximal stimulation of the same receptors which give rise to pressure when stimulated at lower levels of intensity.

While more intense stimulation tended to be correlated with the production of pain and lighter stimulation with the arousal of pressure, and greater amount of stimulation was correlated with slightly increased accuracy of localization, nevertheless the subjective descriptions seem to be chiefly in harmony with the qualitative theory. This was particularly indicated in the work in which the sensations that were aroused by the thistle were classified according to quality into three groups. Although the *O*s experienced difficulty at first in classifying these experiences, they were soon able to make prompt judgments in regard to the class into which the experience belonged. These judgments were made in terms of characteristics which were specific to each of the three designated qualities. The quality of pain was distinguished by greater brightness and liveliness, and by a stinging or penetrating quality. As compared with pain, pressure was always designated duller. Some of the pains were brighter than others, and so were some of the pressures duller than others. However, the linear series of dull pressures, if they

may be said to represent a simple uni-dimensional series, did not seem to be continuous with the series of bright pain qualities. Pressure did not seem to be a pain that had lost its brightness, nor did pain appear to be a pressure that had acquired some degree of brightness. Each of these two qualities was definitely distinguished from the other, and there was little evidence that the two experiences had any characters in common. Furthermore, the pressure-pain complex was not described as an intermediate between pressure and pain, but in general as an experience in which these two distinctive qualities occurred side by side.

Summary

The experimental results which are presented above indicate that stimulations of pain are localized with a slightly greater degree of accuracy than stimulations of pressure. Stimulations of pressure-pain complex are localized, in general, with a degree of accuracy slightly greater than that for either of the simple qualities of pain or pressure. The number of perfect localizations is slightly greater for pain and pressure-pain complex than for pressure, and largest errors of localization tend to be greater in the cases of pressure and pain than in that of pressure-pain complex.

With regard to theory, the suggestion is made that the greater degree of accuracy in localization in the case of pressure-pain complex may find its explanation in the fact that two kinds of impulses are here being integrated in the cortex, whereas in the case of pain or pressure the impulses which arise from stimulation are more homogeneous in character. Although a body of relatively homogeneous impulses pass over the nerve in stimulations of simple pain and simple pressure, it is still possible that there is greater variability in the volley of impulses for pain than for pressure to account for the greater accuracy of localization in stimulations of pain.

AN EVALUATION OF SOME RESULTS FROM THE KNOX CUBE TEST

By W. LINE and M. E. FORD, University of Toronto

This study attempts to evaluate some results obtained by giving the Knox Cube Test to public school children. It is concerned with the possible uses of the test in clinical and educational settings for normal children, and with questions of validity and reliability. It deals also with the relationships between this performance test, the Stanford-Binet scale, and measures of school progress.

Historical review. In 1914 Knox first published his scale for estimating mental deficiency, based upon work with immigrants at Ellis Island.[1] In this battery was included the Cube Test, which seems to have attracted more interest and attention than any other single unit of the scale. Knox claims that the test assumes no teaching; that it measures inherent power, reasoning, judgment, sense of form, perseveration, attention, and coöperation. Since his standardization is based on the results of 400 suspected mentally deficient subjects, and since we are primarily interested in the test in its application to normal children, we are not particularly concerned with the results he then obtained.

The following year Pintner published his revision of this test.[2] He refined the method of presentation and expanded the test, making it more than twice its original length. He then standardized his revision, using 867 normal children, a few adults, and 463 feeble-minded individuals. He writes, "the test appeared to me, at first seeing it applied, to be an excellent one in many ways. Without attempting to enter into a useless discussion as to the actual mental processes involved, we may say in a general way that it depends largely upon imitation, at the same time affording every opportunity for other factors involving intelligence to assert themselves."[3] The basis for this interpretation is not clear from the evidence presented.

In 1918, an examination of freshmen at the Carnegie Institute of Technology, led Rachofsky to make a study of speed of presentation of the test as a factor in ease of recall.[4] He used colored blocks instead of the standard black ones, and allowed

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[1] H. A. Knox, A scale based on the work at Ellis Island for estimating mental defect, *J. Amer. Med. Assoc.*, 62, 1914, 741-747.

[2] R. Pintner, The standardization of Knox's Cube Test, *Psychol. Rev.*, 22, 1915, 377-401.

[3] *Op. cit.*, 377.

[4] L. M. Rachofsky, Speed of presentation and ease of recall in the Knox Cube Test, *Psychol. Bull.*, 15, 1918, 61-64.

his *Ss* (junior and senior boys in high school) two trials on each test. He divided his *Ss* into 7 equal groups. The test was given to the first group at the rate of one tap per half second; with the second group the rate was one tap per three-quarters of a second; with the third, one tap per second; and so on up to the seventh, with which the rate was one tap per two seconds. He concluded that the slower the examiner presents the test the fewer the number of errors. Ease of recall seems to be a continuous function of speed of presentation; but he suggests that if the speed of presentation were slowed down still further, a U-shaped curve would result due to the process of forgetting. He finds that it is more serious to give the test too fast than too slow, and suggests that the best speed for presentation is one tap per one and a half seconds. He finds in his experimentation that imagery is predominantly kinaesthetic, associated with either visual imagery for colours or auditory imagery for color names. A few bright children numbered the cubes. "Incidentally," he says, "one of the explanations of the excellence of the Knox Cube as a mental test is the premium it pays to resourcefulness and ingenuity."[5] These, again, are impressions rather than conclusions based on experimental evidence.

In January of this year, Reymert and Hartman published a study of the Knox Cube Test in which it was their purpose to emphasize the qualitative aspect rather than only quantitative results.[6] The *Ss* used were 50 children between the ages 13–14 yr., and 100 adults between 21–30 yr. These *Ss* were given the test 8 times at two-week intervals. The actual learning from trial to trial was slight, but greatest between the first and second trials. The average intercorrelation of all the trials was 0.36. The first trial correlated least with the successive trials. The number of items would have to be increased 25 times to give a reliability of about 0.90. The qualitative results were indefinite. It was found that procedures for solving the test varied from individual to individual, and in the same *S* from trial to trial. "The most superior method is probably 'the organization of the cubes tapped into a pattern of units' (recall as a whole)."[7] In their general conclusions, the authors say, "in spite of seemingly adverse facts against using the Knox Cube Test as a measure of ability, it may still prove useful in helping to differentiate subnormal levels of general intelligence."[8]

The test has been included in several intelligence scales, such as the Army, Gaw, Pintner-Patterson, Universal, and Knox Performance Scales; and it has long been employed in clinics. Opinions differ, however, as to its value. At the present time the weight of opinion seems to be against the test. Wells writes that "the Knox Cube Test was among the first performance tests to be standardized for age-levels, but it is significant for age-level only as a member of a considerable group of tests. Much depends upon the process of memorizing the pattern. Overt responses give relatively little clue as to what this has been. Visual, auditory, and kinaesthetic processes readily enter. The test may be regarded as one of intelligence in so far as high scores reflect systematic use of such clues."[9]

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[5] *Op. cit.*, 63.
[6] M. L. Reymert and M. L. Hartman, A qualitative and quantitative analysis of a mental test, this JOURNAL, 45, 1933, 87-105.
[7] *Ibid.*, 104.
[8] *Ibid.*, 105.
[9] F. L. Wells, *Mental Tests in Clinical Practice*, 1927, 138.

From the Judge Baker Foundation in Boston comes the following evaluation: "In our experience the variability of performance on repeated trials has led us to believe that the test is unreliable. Having formerly used it extensively, we now employ it very seldom."[10]

It will be seen from the foregoing that comparatively little systematic reseatch has been done with this particular test. The interpretations of its validity that have been given, are, in the main, speculative; and, as with most performance tests, the exact purpose underlying its use is hard to discover. The question of reliability has not received much attention, although Reymert's study suggests it to be low, and attributes the source of unreliability only to the inadequate length of the test. It was with a view to throwing further light upon these characteristics of the test, that the evaluation of results here presented was undertaken.

Method and Procedure

Subjects. The *S*s used in this experiment were pupils at Regal Road Public School, Toronto. They ranged in age from 5 to 15 yr.; and in grade from Kindergarten through Senior IV.[11] When the study was begun, Knox scores were available for 559 pupils, who had been examined by the Mental Hygiene Research Division during the past five years. Some 150 further cases were examined by the writers.

Method of examination. The examinations were conducted with equipment similar to that used by Pintner. This consists of 4 one-inch cubes, and a fifth cube used for the tapping process. The only variation here adopted was that the 4 cubes were mounted on a black strip which held them in a constant positional relationship, 2 in. apart. The strip of cubes is placed on the table between the *E* and *S*. After each line (set of taps) has been given, the fifth cube is placed in front of the child, equidistant between the third and fourth cubes, but nearer the child.

The following 12 lines are Pintner's revision of the original Knox Test, and are the lines used in this experiment:

Line	Blocks tapped	Line	Blocks tapped	Line	Blocks tapped
1	1234	5	1432	9	13124
2	12343	6	1423	10	143124
3	12342	7	13243	11	132413
4	1324	8	14324	12	142341

Instructions. The standard instructions were used. Before the test, *S* was told, "Watch carefully and then do as I do," and then after a line has been tapped by *E*, he was asked to "Do that." In no case tested by the writers was there any confusion or misunderstanding of the directions.[12]

Scoring. The total score on the Knox Cube Test consists of the number of lines correctly imitated.

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[10] A. F. Bronner, W. Healy, G. M. Lowe, and M. E. Shimberg, *A Manual of Individual Mental Tests and Testing*, 1928, 196.

[11] In the Toronto Public Schools there are 7 grades above the Kindergarten, called respectively Junior and Senior First, Second, Junior and Senior Third, Junior and Senior Fourth.

[12] Pintner suggests that the Knox Cube Test would be equally effective if the directions were entirely omitted. He cites a few cases where no directions were used, but where the test was given with satisfactory results (*op. cit.*, 378).

Results

(1) Relationship between Knox Cube scores and chronological age. The average total Knox Cube scores for the various ages are given in Table I. The data on which this table is based were selected from the case-histories at the school, and from the results of tests administered by the writers, so that 50 children of each age-group were included; and also so that the age-averages for the groups fall at exactly 6, 7, 8, etc. yr. The chronological and mental age ranges were also made comparable from group to group by this method of selection.

Table I indicates an increase in the average number of lines passed from the younger to the older age-levels, but it is evident that this increase is very gradual. There are 9 age-levels, and only a differentiation of 4 in the average number of lines passed from the younger to the older

TABLE I

Relation between Knox Cube Scores and Age

Chronological Age		Mental Age		No. Lines Passed			Pintner's Results
Av.	Range	Av.	Range	Av.	Range	S.D.	
6	5·7–6·6	6·2	3·7–7·8	3.36	0–8	1.82	4.22
7	6·7–7·6	7·0	4·6–9·1	5.34	2–9	1.78	5.12
8	7·7–8·6	7·11	4·9–10·0	5.22	1–8	1.53	5.60
9	8·7–9·6	9·0	6·4–11·0	6.20	3–11	1.63	5.59
10	9·7–10·6	9·11	7·4–12·6	6.02	2–10	1.79	6.29
11	10·7–11·6	10·10	7·11–13·1	6.14	3–11	1.65	6.68
12	11·7–12·6	12·0	8·6–14·2	7.04	3–10	1.51	6.66
13	12·7–13·6	12·11	9·2–14·8	6.64	3–10	1.44	6.66
14	13·7–14·6	13·11	10·8–16·0	6.92	3–10	1.36	7.55

levels. This would seem to suggest that the material is not finely graded in difficulty. It is interesting to note that these findings agree closely with those obtained by Pintner.

(2) Relationship between Knox Cube score and mental age. The Stanford Revision of the Binet Test was given to each pupil together with the Knox Cube Test at the same interview. The distributions of mental ages and Knox scores appear to be fairly normal, as the following data show.

Binet Mental Age	Frequency	Knox Scores	Frequency
3 and 4 yr.	6	0 and 1	7
5 and 6 yr.	51	2 and 3	46
7 and 8 yr.	115	4 and 5	119
9 and 10 yr.	119	6 and 7	191
11 and 12 yr.	92	8 and 9	75
13 and 14 yr.	57	10 and 11	7
15 and 16 yr	5		

Correlation coefficients were calculated between mental age and Knox Cube score for the age-groups separately, using the Pearson product-moment formula. The results were as follows.

Age	r	P.E.	Age	r	P.E.	Age	r	P.E.
6	0.41	0.08	9	0.31	0.09	12	0.25	0.09
7	0.38	0.08	10	0.32	0.08	13	0.41	0.08
8	0.25	0.09	11	0.48	0.07	14	0.41	0.08

These coefficients of correlation are all positive, and would, therefore, seem to indicate that there is some relationship between Knox Cube score and mental age. The correlations, however, are relatively low, and no definite relationship appears between the size of the correlation and the age-groups. It is true that the range of talent in each group is comparatively small; but even when this is offset by correlating the entire group, the correlation becomes only 0.58, P.E. 0.02.

(3) Relationship between school grade and Knox Cube score. Approximate measures of relationship were sought by correlating the score in the Knox Cube Test with school grade at the time the test was given. The values obtained are compared in Table II with similar values for the mental test.

TABLE II

CORRELATION OF THE SCHOOL GRADE WITH KNOX CUBE SCORES AND WITH MENTAL AGE

Age-group (in yr.)	Knox Cube score and school grade		Mental age and school grade	
	r	P.E.	r	P.E.
6	+0.21	0.09	+0.37	0.08
7	+0.33	0.09	+0.55	0.07
8	−0.48	0.07	+0.63	0.06
9	+0.42	0.08	+0.72	0.05
10	+0.16	0.09	+0.59	0.06
11	+0.43	0.08	+0.67	0.05
12	+0.11	0.09	+0.49	0.07
13	+0.22	0.09	+0.51	0.07
14	+0.09	0.09	+0.67	0.06
All	+0.39	0.03	+0.73	0.01

It will be noted that the coefficients obtained with scores in the Knox Test are decidedly lower, and much more variable, than those obtained with the mental test.

(4) Reliability of the Knox Cube Test. It is difficult to obtain a measure of reliability on this test without actually repeating the test. Since this was impracticable with the group here under consideration, the method

of correlating scores on odd and even lines was adopted. While, particularly with a test of this length, this is not very adequate, the results are given in Table III, and some further indication of reliability appears in a later section. It will be noted that the values given in Table III vary considerably, and are all low.

TABLE III

COEFFICIENTS OF RELIABILITY
(Correlating odd and even lines)

Age-group (in yr.)	r	P.E.
6	0.42	0.08
7	0.48	0.07
8	0.39	0.08
9	0.35	0.08
10	0.46	0.07
11	0.25	0.09
12	0.28	0.09
13	0.47	0.07
14	0.22	0.09
All	0.28	0.03

(5) Difficulty of the test material. Lack of differentiation in score for the different age-levels suggested that it might be fruitful to examine the material for difficulty value. Table IV shows the percentage passing each line of the test at the different age-levels.

The results are irregular. For instance, we find that line 4 of the test seems more difficult for 9-yr. old children than line 5, while the reverse

TABLE IV

PERCENTAGE PASSING EACH LINE OF THE TEST AT THE DIFFERENT AGE-LEVELS

Age (yr.)	Line											
	1	2	3	4	5	6	7	8	9	10	11	12
6	92	66	54	26	40	18	8	10	8	4	0	6
7	100	96	80	50	66	58	14	22	26	6	2	12
8	98	94	94	52	58	60	12	20	24	2	0	6
9	100	100	92	68	80	68	30	10	30	6	6	4
10	100	96	92	72	64	78	42	16	20	8	12	4
11	98	100	92	70	78	80	38	28	20	8	4	12
12	100	98	92	78	76	82	48	38	28	20	20	12
13	100	100	92	74	78	88	44	30	26	6	14	4
14	100	100	98	80	78	95	58	30	44	8	14	14

is true for the 10-yr. group. Likewise, line 11 seems easier than line 12 for the 10-yr. group, while at 11 yr., line 12 appears to be easier than line 11.

If we transpose the percentages of Table IV into sigma values ranging from —3 sigma to +3 sigma, as has been done in Fig. 1, an interesting

fact is brought out. While there is fluctuation in the difficulty value of the lines, nevertheless there seem to be four fairly definite groups, namely, 1, 2, and 3; 4, 5, and 6; 7, 8, and 9; 10, 11, and 12. Although these lines change around on the scale at different age-levels this difficulty grouping seems to be constant. This is more definitely reflected when we combine all the age-groups.

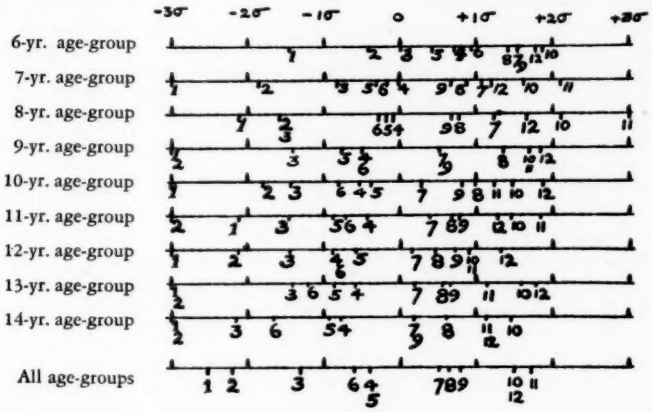

FIG. 1. SIGMA DIFFICULTY SCALE
The difficulty of every line of the Knox Cube Test is shown in sigma values for every age-level.

Analysis of the lines themselves also seems to point to the same grouping. The first 3 lines are 1234, 12343, and 12342. The next 3 lines are 1324, 1432, and 1423 (means and extremes). The next 3 lines are 13243, 14324, and 13124 (five digits). The last 3 lines are each composed of 6 digits.

These values seem to point to the fact that we have in the Knox Cube Test material about 4 levels of difficulty. If this is the case, then the test is, at best, only a very crude instrument for measuring. It may differentiate the extremes of the group, but it is useless for fine distinctions. No doubt a test of 4 difficulty levels serves the purpose for which this test originated—to distinguish feeble-minded from normal individuals. Such a test is not, however, of very great value when applied to school children, if interpreted merely on the basis of the total score obtained.

(6) Analysis of errors. A qualitative study of the nature of errors was undertaken to see if the types of errors made might be classified, and if

such a classification might reveal data regarding the individuals tested. The procedure adopted was to determine the frequency with which any particular faulty response was made at the various age-levels; and the results of thus analysing the responses are summarized in what follows.

From the seventh line to the end of the test, no definite pattern of errors was recognizable. Apparently the individual got the pattern of the test line as a whole, and reproduced it correctly, or else failed to do so and gave only a random response.

At the 5-yr. level the outstanding characteristic seemed to be to repeat the first line (1234) as a response to subsequent tests. This error seemed to be again indicated on line 2 at the 6-yr. level, but dropped out for the older levels.

Line 4 (1324) revealed an error pattern of 1342, indicated as early as the 5-yr. level, and carried through to the 14th. yr. At 5 yr., 5 out of the total of 23 errors made on this line were of this pattern. This error increased in frequency until at the 14-yr. level 5 out of the 7 errors made were of this type.

Line 5 (1432) revealed an error pattern of 1423 in 4 out of 19 errors at the 5-yr. level, and the pattern increased in frequency until at the 14-yr. level, 6 out of the 7 errors were of this type.

Line 6 (1423) showed an error pattern of 1432 in 13 out of 28 errors made at the 5-yr. level, while the only error made at the 14-yr. level was of this type. This is an interesting error especially because 1423 is the correct response for the test line immediately preceding.

From giving the test, two general observations may be made: (1) at the lower age-levels, there was a noticeable tendency to respond to a given line by a series of random hits rather longer than the given line; while at the higher age-levels, the wrong responses tended to be shorter than the test line; (2) recognition of the *S*'s failure to reproduce a line correctly was realized by the *S* more often at the older levels. Any response seemed to give satisfaction to the younger children, while the older were more self-critical.

The main indication of the above data is that a systematic construction of patterns of response, with careful attention to the relations involved, might lead to a more adequate scale of difficulty, and to increased knowledge of the nature of the processes involved in performing these test-items.

(7) Examination of a group of Third Grade pupils. In selecting a group of *S*s for more intensive examination, the Third Grade level was chosen. The results of the correlations quoted above did not indicate any particular level of the school where Knox scores would have greater significance than at other levels. Pupils from a Junior and a Senior Third class were therefore employed purely as a matter of convenience.

Seventy-five cases were tested with the Knox Cube Test,[13] and for these

[13] Exploratory experimentation had previously been undertaken by the writers with a view to seeing if the technique of giving the test could be improved (a) by

were available recent scores on the National Intelligence (Group) Scale. The latter had been given three months earlier, and the mental age scores were accordingly calculated as on the date of giving the Knox. The measures of school progress employed were those deemed to be of greatest importance at this level; namely, arithmetic, literature, reading, and writing. The scores on all six variables were translated into sigma deviation units from the mean of the class, and the intercorrelations are shown in Table V.

TABLE V

INTERCORRELATIONS OF THE KNOX CUBE SCORES, INTELLIGENCE, AND SCHOOL MARKS

	(A)	(B)	(C)	(D)	(E)
(A) National					
(B) Knox Cube	−.09				
(C) Literature	.33	.12			
(D) Arithmetic	.48	.06	.32		
(E) Reading	.23	−.02	.56	.17	
(F) Writing	.04	.11	.39	.29	.06

Preliminary analysis of this table by Thurstone's Multiple Factor Method[14] suggested that the major factor underlying the National Test and school marks was less critically represented by the Knox scores; and that probably some other factor was more important in the latter case. Accordingly it was decided to add to the above table, correlations involving certain other psychological tests of known validity, and to analyze the results for other factors. In this way some indication might be obtained of the objective validity of the Knox test.

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varying the time of presentation, and (b) by varying the method of scoring. In regard to the former, a group of 44 children were selected from Junior and Senior Third. This group was divided into two sub-groups approximately equal in age, grade standing, and intelligence as measured by the Stanford-Binet. The first group was given the Knox Cube Test at the usual speed of one tap per second. The second group was given the test at the speed of one tap per half second. In neither case was *S* controlled in his speed of response. The results indicated that as far as score was concerned, there was no difference due to the time of giving. The range of scores in both cases was 4 to 9. The average scores of the two groups were 6.8 (sigma 1.89) and 7.0 (sigma 1.58) respectively. Because it would be necessary to experiment extensively and to make a new standardization before we could be justified in changing the speed of presentation, we decided to use the standard speed in this experiment.

Concerning the method of scoring, several revisions were considered: (1) weighting the lines according to difficulty; (2) counting the number of correct digits in the correct patterns; (3) counting the number of correct digits in correct patterns plus digits correct (in the right place) in incorrect patterns; and (4) counting the number of correct patterns plus (relative-degree-of-accuracy) score in incorrect patterns. The fact that there appeared to be only four levels of difficulty in the test suggests that no such revisions would constitute significant improvement, unless a more finely graded scale were first constructed. In the experiment being discussed above, the standard scoring method was employed.

[14] L. L. Thurstone, Multiple factor analysis, *Psychol. Rev.*, 38, 1931, 406-427.

The psychological tests included, were (a) tests of 'perseveration,' and (b) tests of 'speed of execution.' The former consisted of a battery of four tests such as were used by Pinard;[15] the latter involved the speed of writing numbers, letters, etc., in known sequences, such as 1, 2, 3, 4, etc. These batteries had proved to involve factors other than *g* (as indicated by Spearman's tetrad technique) and to have high reliability.

The additional correlations thus obtained were as follows:

	(A)	(B)	(C)	(D)	(E)	(F)	(G)
(G) 'Perseveration'	.28	−.07	−.01	−.03	.02	−.26	
(H) 'Speed of Execution'	−.02	.04	−.04	.16	.11	.15	.11

Thurstone's technique then gave the following loadings of factors underlying these eight variables.

	Factor		
	I	II	III
(A)	.57	.40	.22
(B)	.34	−.51	.11
(C)	.78	.17	−.14
(D)	.67	.15	−.29
(E)	.57	.21	.30
(F)	.54	−.04	−.40
(G)	−.20	.83	.33
(H)	.23	−.12	.70

While the unreliability of the Knox Cube scores, and the comparatively unknown nature of school marks, make this form of analysis very questionable, with the present data—particularly since Thurstone's technique offers as yet no way of determining the probable-error values of the 'loadings'—some preliminary indication of what Knox scores represent may be read into the results. The Knox Test appears to measure the first factor less adequately than does the National Test, and than it measures the second factor. The first factor is presumably *g*; the second may conveniently be designated *p* ('perseveration'), although it will be noted that Knox scores are inversely related to the perseverative tendency. 'Speed of execution,' the third factor, seems to bear little relationship to this test.

(8) Relationship between Knox Cube scores and misdemeanors. The misdemeanor records of the group of 75 Third Grade children were studied to see if score on the Knox Test was related to frequency and nature of misdemeanors.[16] The Knox Test results were divided quite arbitrarily into

[15] W. Pinard, Tests of perseveration, *Brit. J. Psychol.*, 23, 1933, 5-19.

[16] These records are based on the daily observation of classroom behavior. Their form has been reported elsewhere (W. E. Blatz and E. A. Bott, Studies in mental hygiene, *Ped. Sem.*, 34, 1927, 552-582).

3 groups. The following results came from a study of misdemeanors according to these groupings:

Knox score	Number of cases	Frequency of misdemeanors	Misdemeanors per capita
9–11	11	30	2.72
6–8	45	55	1.22
3–5	19	34	1.79

This seems to indicate that children who obtain extreme scores on the Knox Test show higher frequency of misdemeanors in the classroom. The group scoring high on the test has the greatest number of misdemeanors per capita.

Misdemeanors which could be listed under the classification of "lack of application" *e.g.* restlessness, inattention, carelessness, laziness, day dreaming, etc.) were next studied, with the following results:

Knox score	Number of cases	Frequency of misdemeanors	Misdemeanors per capita
9–11	11	12	1.09
6–8	45	17	0.38
3 5	19	16	0.84

Here again, children with extreme Knox scores show higher frequency of misdemeanors of this type.

Conclusions

(1) The Knox Cube Test gives scores which increase slightly with age.

(2) There is a positive but low correlation at every age-level between Knox scores and Binet mental age.

(3) The relation between Knox scores and school grade is variable and never as high as the relation between Binet mental age and school grade.

(4) The reliability of the test is low.

(5) There are approximately only four difficulty levels in the material of the test.

(6) Preliminary analysis indicates that the Knox Test measures general intelligence, but only poorly. It suggests the presence of a second factor which is related to 'perseveration.'

(7) There is some indication of a relationship between behavior and the Knox Test in that the extremes on the test show higher frequency of misdemeanors.

THE RELATION OF SATURATION TO THE SIZE OF THE RETINAL IMAGE

By S. SMITH STEVENS, Harvard University

It has been observed that the saturation of visual sensation changes with an alteration of the size of retinal area stimulated and also with the position of the image upon the retina. The variation of saturation with the size of the stimulated area was noted by Charpentier in 1886.[1] He reported that, if a red and a green are placed side by side and matched for intensity, a change in their relative values follows upon any subsequent change in the size of the field occupied by the two colors. If the field is enlarged the green appears brighter, and *vice versa.*

The phenomenal changes in colors accompanying a shift in the position of the retinal image was investigated by Whitman in 1899.[2] He arranged an apparatus permitting the exposure of Milton Bradley colored papers at various angles with the line of regard, and he measured photometrically their relative brightnesses. Using the brightness of yellow-green, which remained constant at all angles, as the unit of measure, he found the relative values of red and green-blue to be as follows:

	0°	30°	50°	70°
Red	.514	.275	.194	.165
Green-blue	.529	.680	.717	.717

In other words, as a red stimulus is moved from the center to the periphery of the visual field, there is a decrease in brightness; but if the stimulus is green-blue, an increase in brightness results.

Another but closely related aspect of this phenomenon is the observation by Troland[3] that the Purkinje phenomenon does not hold in the fovea itself, but it reversed. There are many differences between foveal and peripheral vision reported by numerous investigators, most of whom convey the impression that the area exhibiting the characteristics of foveal vision ends abruptly where the periphery begins. Their assumption seems to be

* Accepted for publication June 18, 1932.

[1] Aug. Charpentier, Sur une condition physiologique influençant les mesures photometriques, *Lumière électrique,* 21, 1886, 165.

[2] F. P. Whitman, On the brightness of pigments by oblique vision, *Science,* 9, 1899, 734-736.

[3] L. T. Troland, The absence of the Purkinje phenomenon in the fovea, *J. Franklin Institute,* 182, 1916, 111 f.

that, granted an adequate technique, a sharp line of demarcation could be drawn around the fovea. The results of the present experiment lead one to question this notion.

The author began the investigation at the suggestion of Professor Boring with the aim of discovering the causes and the exact nature of a phenomenon common enough to those experienced in the use of rotating color-mixers. It is readily observed that in mixing the complementaries, red and blue-green, the resulting gray is pure only if one stands at a certain distance from the color-wheel. Any change in this distance results in a change in the gray, which becomes reddish as the distance increases or grows bluish-green as one approaches the color-mixer. The relation of this phenomenon to that observed by Charpentier is obvious; its relation to the experiment by Whitman is a little less obvious but no less intimate; and its importance for the theory of the retinal mechanism is vital.

Technique

A motor-driven color mixer, set behind a black screen which included an Aubert diaphragm, constituted the essential apparatus. The color-wheel consisted of a large disk, 11 in. in diam., on which were fastened the Milton Bradley colored papers red and blue-green, with 180° of each color exposed. Covering the center part of this large disk was a smaller one, 5¼ in. in diam., made up of 300° of black and 60° of white paper. Rotation of the disk, then, produced a gray by reason of the red and blue-green mixture and also a gray by means of the black and white mixture. The latter gray served as a criterion for the purity of the former.

The color-wheel was illuminated by a cluster of 3 lamps, one 75-watt and two 60-watt, type C-2, Mazda lamps, suspended behind the screen slightly above the aperture. It was found necessary to use direct current in these lamps to avoid the stroboscopic shadows.

The screen was set 8 in. in front of the color-mixer. The size of the Aubert diaphragm, through which the *O* could see approximately equal amounts of both the larger and smaller disks, could be regulated by the pulling of two strings held by the *O*.

The *O*'s chair was set upon a miniature 'flat car' mounted on a track, which made it easy to alter his distance from the screen. A constant position of the *O*'s head was assured by a head-rest. The diaphragm and the flat car provided, then, two means of changing the size of the retinal image which resulted from the *O*'s fixating of the color-wheel.

The *O*'s *Aufgabe* was either that, given a certain position in front of the screen, he should change the size of the aperture until a homogeneous field resulted (until the gray of the inner and the outer disks was indistinguishable), or that, given a certain sized aperture, he should alter his distance until the grays were matched. Both methods gave thoroughly consistent results, but the latter procedure was subjectively easier and yielded a relatively smaller probable

error in successive judgments. Therefore, in the major part of the experiment, the aperture was fixed at 1.0, 2.5, and 4.0 in. for the side of the square opening, and the *O* was required to determine the distance at which the best match occurred. This judgment is not at all difficult, since the gray of the smaller disk assumes a tinge of contrast color whenever the color of the larger disk is altered by a change in the *O*'s position. As the *O* recedes from the screen the color of the larger disk becomes reddish and that of the smaller one turns into a bluish green. Approaching the screen causes a reversal of this effect.

The next step was to introduce into the black and white disk a small sector of red or blue-green and to note again the distance at which the *O* experienced a homogeneous field. The red sector increased, while the blue-green sector decreased, this distance by an amount dependent upon the size of the sector. It is then possible to compute the size of the retinal image which obtains when the *O* experiences a homogeneous field.

Twenty-seven *O*s, mostly university students, were used. Two of these were color-blind and could not observe the phenomenon; but from the remaining 25 it was possible, incidentally, to determine the distribution of the effect in such a sample, as measured by the distance from the screen at which the individuals were able to report a homogeneous field. The resulting distribution curve resembles closely the Pearson type III, being positively skewed. The more thorough data, however, were obtained from 2 *O*s of whom the author (*St*) was one and P. M. Zoll (*Z*) the other.

Results

Two facts are evidenced by the data obtained. (1) The saturation and hue of a visual experience is dependent upon the size of the retinal area stimulated, and upon its position. (2) For a constant increment of change in saturation a constant proportion of the area stimulated must be added.

The first point is merely a verification of facts already known, but the second is an expression of the nature of the function which holds between saturation and the size of the retinal image. The results from which this function is derived are shown in Tables I and II.

The color increments added to the center disk in Table II are twice as large as those of Table I. Therefore in order to make the ratio $\Delta a/a$ comparable for the two cases, the former should be divided by 2. The meaning of the difference between these two ratios is discussed below in connection with the slopes of the curves of Fig. 1.

The dimension of the retinal image for each increment of color in the center disk is the average of 18 observations in the case of *St* (6 for each size of aperture) and 6 observations in the case of *Z*. The author repeated the observations on himself using colored papers of slightly different saturation and brilliance and obtained the same type of function, as is shown by Fig. 1. All observations were made in a random order.

TABLE I

DIMENSIONS OF THE RETINAL IMAGE OBTAINED FROM *St* FOR DIFFERENT AMOUNTS OF BLUE-GREEN AND RED IN THE CENTER DISK IN ORDER TO MATCH THE INNER AND OUTER DISKS

($\Delta a/a$ is the ratio of the decrement in area, for each decrement of blue-green, to the original area of the retinal image)

Color of center disk	Amount	Distance in in. of *O* from screen for apertures			Av. diam. retinal image (in mm.)	One-half area of retinal image (in mm.^{2})	$\Delta a/a$ Av. = .412
		1 in.	2.5 in.	4 in.			
Blue-green	24°	2.7	6.7	11.0	5.535	15.318	
	20°	4.3	10.0	13.0	3.951	7.805	.490
	16°	7.0	12.5	17.8	2.837	4.024	.484
	12°	9.8	17.8	26.7	1.961	1.923	.522
	8°	11.5	23.8	33.5	1.557	1.212	.368
	4°	13.5	28.3	40.0	1.312	.861	.289
	0°	17.5	33.2	48.3	1.076	.579	.327
Red	4°	22.8	45.8	63.7	.805	.324	.440
	8°	31.7	59.8	83.0	.607	.184	.432
	12°	39.8	83.0	120.7	.441	.097	.472
	16°	62.7	112.7	169.3	.308	.047	.515
	20°	77.0	146.0	209.7	.245	.030	.361
	24°	90.3	175.0	257.7	.204	.020	.333
	28°	120.0	222.7	316.3	.164	.013	.350
	32°	138.0	242.0	420.0	.127	.008	.384

TABLE II

DIMENSIONS OF THE RETINAL IMAGE OBTAINED FROM Z FOR DIFFERENT AMOUNTS OF BLUE-GREEN AND RED IN THE CENTER DISK IN ORDER TO MATCH THE INNER AND OUTER DISKS

($\Delta a/a$ is the ratio of the decrement in area, for each decrement of blue-green, to the original area of the retinal image)

Color of center disk	Amount	Distance in in. of *O* from screen for apertures			Average diam. of retinal image (in mm.)	One-half area of retinal image (in mm.^{2})	$\Delta a/a$ Av. = .423
		1 in.	2.5 in.	4 in.			
Blue-green	24°	9.5	26.0	40.0	1.507	1.136	
	16°	14.0	42.0	59.0	1.000	.500	.528
	8°	18.5	59.0	83.0	.759	.288	.424
	0°	30.5	58.0	96.0	.556	.155	.461
Red	8°	39.0	86.0	131.0	.426	.091	.413
	16°	44.0	101.0	190.0	.342	.058	.362
	24°	57.0	145.0	232.0	.260	.034	.413
	32°	80.0	180.0	260.0	.208	.022	.363

Most significant of these results is the constancy of the ratio between the decrement in area necessary to neutralize a given increment of red in the center disk (Δa) and the area already under stimulation (a). This ratio shows that a logarithmic relation holds between the size of the retinal

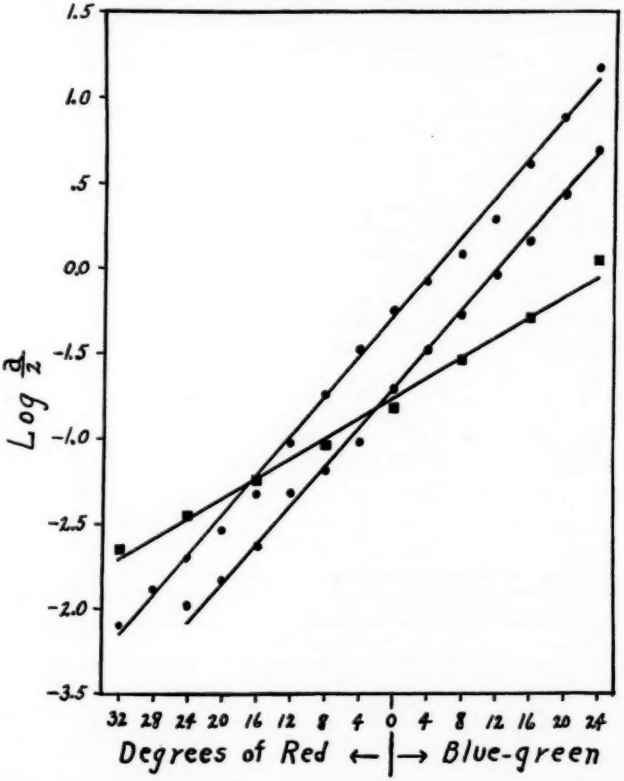

FIG. 1. THE RELATION BETWEEN THE AREA OF THE RETINAL IMAGE AND THE SIZE OF THE RED OR BLUE-GREEN SECTOR IN THE CENTER DISK OF THE COLOR WHEEL
The square points are for *Z*'s data; the others are for *St*'s. The lower line for *St* is from observations not tabulated in this report.

image and the amount of blue-green in the center disk. It is assumed that a given sector of blue-green is equal to the absence of the same amount of red and *vice versa*, since equal amounts of the two colors are exactly complementary under the light used. In other words, this relationship means

that the retina is relatively more sensitive to red than to blue-green at the center and that this sensitivity decreases, very rapidly at first, as one goes towards the periphery, and more slowly as one progresses outwards. The function representing the sensitivity of the retina to blue-green is the converse of that for red, as shown in Fig. 2. As the image spreads farther

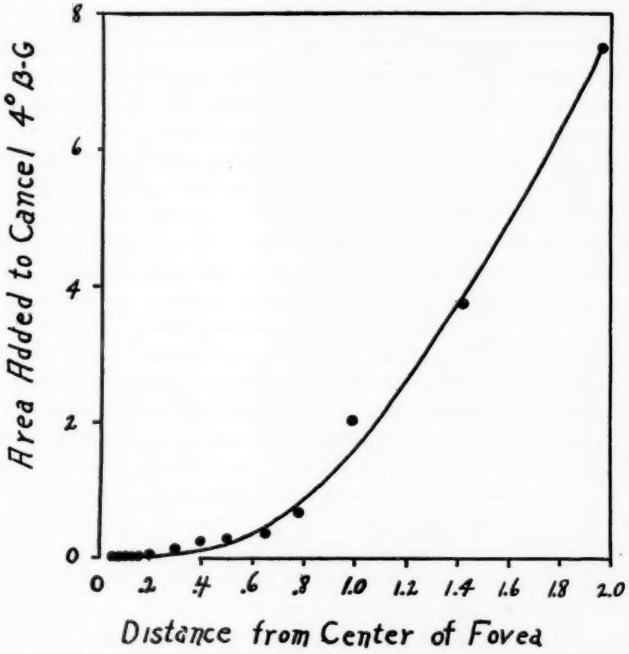

FIG. 2. CURVE SHOWING THE NATURE OF THE DISTRIBUTION OF COLOR SENSITIVITY IN THE RETINA OF *S1*

The points represent the observed increments in area which must be added to balance the effect of an increment of 4° of blue-green in the center disk, when the area covered by the retinal image extends beyond certain distances from the center of the fovea.

from the center of the fovea the amount of additional area needed to balance an increment of blue-green added to the center disk becomes greater, but, over the wide range tested, this amount is found to be a fixed proportion of the original area. The differential sensitivity of the retina to color follows a simple logarithmic law. This fact is easily observed qualitatively if one but shifts his point of fixation while looking at the

homogeneous field of the color-wheel. A very slight angular shift causes the outer disk to become bluish-green and the inner one to become reddish by contrast; but, as the fixation point becomes farther removed from the line between the inner and outer disks, the effect of additional angular increments grows less and less. The importance of this fact in relation to the problem of the anatomical distribution of cones is obvious.

It will be noted from the data of Table I that the logarithmic relation holds for measurements which take us well within the fovea. (Table II does not show such small areas; the acuity of the *O* was poor). If we assume that the foveal diameter is about 0.3 mm., it is evident that, since the last four determinations are for areas smaller than the total area of the fovea, the logarithmic function reveals no sudden break or change which might be construed as marking off the limits of the fovea. Likewise there is no break at the boundary of the rod-free area, but only a continuous function over the range from 5.5 mm. diam. to 0.127 mm. diam. (Fig. 2). The conclusion follows that the peculiar distribution of sensitivity which accounts for this function is not only a peripheral phenomenon, as is usually held, but is also characteristic of the fovea itself.

The validity of Hecht's[4] assumption that the fovea contains equal numbers of the primary receptor units for blue, green, and violet would appear to be called in question by the present findings. If Hecht's hypothesis were true, and the numbers of the primary cones within the fovea were equal, there would still remain the question of their arrangement. The nature of the functions found in this experiment indicate that there would have to be a massing of the receptors for red at the center of the fovea with a decrease in their density as one approaches the periphery. Then it would follow that the receptors for red within that small central area of about 0.1 mm. diam. would outnumber the receptors for blue-green in the same area, *if* the numbers of the two receptors within the *entire* fovea were equal.

The slopes of the two curves (Fig. 1) obtained from *St* are approximately equal, as is to be expected, but the slope of *Z*'s curve is different. This means that when a given increment, say, 8° of blue-green, is added to the center disk, the proportional increment in retinal area necessary to cancel it is greater in *St*'s than in *Z*'s case. (This is also shown by the magnitudes of the two ratios $\Delta a/a$, in Tables I and II.)

In other words, the variability of sensitivity as one goes from the fovea to the periphery is greater for *Z* than for *St*. If one assumes that the perception of color is due to specific receptor units, these differences in slope

[4] Selig Hecht, The retinal processes concerned with visual acuity and color vision, *Howe Lab. of Ophthal. Bull.*, 4, 1931, 1-54.

would mean that the density per unit area of the cones for red and blue-green shows greater variability from place to place in Z's retina. Or, in terms of the chemical process involved, it would seem to mean that in his case the absorption coefficient for red is less uniform over the whole retina.

THEORY

Two general types of hypothesis can be offered for the explanation of this phenomenon. One possibility is that the visual experience depends directly upon the point at which a dynamical equilibrium is reached within the area stimulated. Thus, it might be that when a large area is affected, the resulting equilibrium is such that the predominating sensation is blue-green. Here an analogy suggests itself: the point of equilibrium of a chemical solution depends upon the concentration of the ingredients. In the same way stimulation of a larger area may render available proportionately more of some substance like rhodopsin, which will cause a change in concentration and a consequent displacement of the point of equilibrium of the reversible photochemical reaction, resulting, thereby, in a blue-green rather than a red sensation.

The other kind of hypothesis takes account of the individual cones and explains the phenomenon in terms of the anatomical distribution of distinct functioning elements, with no reference to their interaction. The picture presented in this case is relatively simple. However, Adrian and Matthews[5] conclude from their work on the eel that the retina cannot be thought of as a mosaic of sensitive points, each connected with its own nerve fiber and acting independently of the rest. To the present author, therefore, it seems inevitable that the truth of the matter lies somewhere between the extremes of these two hypotheses, or at any rate that the best hypothesis for the present is one recognizing the effect of both the nature of the anatomical distribution of cones and the possibility of their mutual interaction after the manner of *Gestalt* dynamics.

In addition to the assumption of separate functional units peculiarly distributed on the retina and capable of interaction, the question of threshold must be reckoned with. To the author it seems well, in view of the following facts, to accept the theory of a frequency distribution of thresholds as put forth by Hecht[6] and to assume, in addition, that the form of the distribution is similar, within a given area, for the cones for red and

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[5] E. D. Adrian and R. Matthews, The action of light on the eye, *J. Physiol.*, 63, 1927, 378-414.

[6] Hecht, *op. cit.*, 4-6.

for blue-green. The evidence for this latter hypothesis is the fact that a change in the intensity of the illumination reaching the color-wheel causes no loss of homogeneity to the field seen through the aperture. If, when the *O* is at a distance from the screen that will render the match between the grays of the inner and outer disks as perfect as possible, the intensity of illumination is decreased by moving the source away from the color-mixer or increased by reflecting light back on to the disks by means of mirrors, then the differential effect, if any, on the two grays is unobservable. This result is found for an intensitive range equivalent to halving and doubling the original intensity.

There arises, however, a very interesting effect if the intensity of only a small part of the field seen through the aperture is altered. Such a change can be made easily by having the *O* fixate a small black disk of about ½ in. diam. held in the center of the aperture in such a manner that it covers equal amounts of the inner and outer disks on the color-wheel. Then, when the small disk is removed, a white negative after-image is seen against the two parts of the field with the result that within the area of the after-image there is no longer a homogeneous gray, but the outer disk is seen as having a reddish tinge, just as would be the case if the size of the aperture itself were reduced. This effect must be due to intensity alone, because it is the same when a small bright spot of light, reflected from the illuminating source by a small mirror, is thrown back on to the revolving disks. Furthermore the phenomenon is more striking when the bright spot is small. Just as with the variation of the aperture itself, the degree of the predominance of red varies inversely with the size of the area stimulated.

Theoretically we can take these facts to mean that, at the original intensity of the color-wheel, not all of the cones are being stimulated, and that, as the intensity in any part of the field is increased, more of these inactive cones reach their threshold and begin to function. If the increase in intensity is applied to the center of the fovea where the receptors for red predominate in number, there will be more thresholds crossed for red than for blue-green, and red will be the predominating experience.

Conclusion

The factual findings of this experiment are:

(1) In mixing colors on a rotating color-mixer the saturation and hue of the visual sensation depend upon the size and the position of the retinal area stimulated.

(2) The relation between the saturation and the area of the retinal

image is a logarithmic function. If saturation changes by constant increments, the area must change by proportional increments.

(3) This function is continuous over a wide range and extends to a point well within the accepted anatomical limits of the fovea.

(4) The center of the fovea is relatively more sensitive to red than to blue-green, but at the periphery sensitivity to blue-green is relatively greater.

(5) Changes of intensity in the illumination of the entire field have no effect on the relative values of red and blue-green over the range tested, but changes within a small portion of the field have a differential effect upon the predominance of the two colors.

The theoretical assumptions supported by these facts are:

(1) Red and blue-green affect different types of cones or primary receptor units.

(2) These functional receptor units vary in density in different parts of the retina; the red is more concentrated at the center of the fovea and less concentrated in the peripheral areas, as compared with the blue-green.

(3) These units have intensitive thresholds of which the distribution is essentially the same for the two colors.

ANTICIPATORY PLACE-SKIPPING TENDENCIES IN THE MEMORIZATION OF NUMBERS

By MILDRED B. MITCHELL, George School, Pa.

PART I

Ebbinghaus found a certain amount of remote forward association in memorizing lists of nonsense syllables.[1] His method of demonstrating this was to learn lists of syllables made from lists previously learned. For instance, a list made from every third syllable of lists previously learned was learned more quickly than the original lists, but less quickly than a list composed of every second syllable of previously memorized lists. Thus he found that associations were made not only between a given syllable and syllables located later in the list, but that the strength of these associations varied inversely with the distance.

Cason did not find any evidence for forward associations.[2] After learning a difficult prose passage, his *Ss* had just as hard a time learning a list of words composed of every other word in order from the passage (as compared with a list made up of separated words) as they did before learning the passage. After learning an easy prose passage or poetry, his *Ss* showed even greater difficulty in learning the lists of words composed of every other word in order from the passage (as compared with a list made up of separated words) than they did before learning the material. He found little evidence for backward associations.[3] After the rote memorization of lists of nonsense syllables and lists of familiar words, he found it in two out of three cases. He found an inhibition, or greater difficulty in learning the lists, after the memorization of logical prose material. In the lists of familiar words and nonsense syllables, the backward associations probably occurred as errors during the learning of the lists. If the same kind of material had been used for testing forward associations, similar results might have been obtained.

Hall used lists of 10 nonsense syllables.[4] The test material was composed of the odd-numbered syllables from one previously learned list and the even-numbered syllables from another previously learned list. Thus the syllables kept their same relative positions and the syllables from the same list were always separated by just one syllable from another list. When the test lists were given immediately after the learning of the original lists, there was no reliable difference between

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[1] H. Ebbinghaus, *Über das Gedächtnis*, 1885, 130-151.

[2] H. Cason, Specific serial learning: A study of remote forward association, *J. Exper. Psychol.*, 9, 1926, 315-316.

[3] Cason, Specific serial learning: A study of backward association, *ibid.*, 9, 1926, 226-227.

[4] M. E. Hall, Remote associative tendencies in serial learning, *J. Exper. Psychol.*, 11, 1928, 65-76.

the learning of these lists and the learning of the control lists. When the learning of the test material was postponed for a week, however, there was a reliable saving in the learning of the test lists as compared with the learning of the control lists. This work, therefore, harmonized the apparent differences between the results of Ebbinghaus and those of Cason. Cason had tested his immediately and Ebbinghaus had relearned his material after 24 hrs. Hall suggested that a moderate original learning with a period of forgetting may be necessary for the functioning of remote associative tendencies.[5]

Lumley,[6] using mazes and series of numbers or letters, found that many errors were anticipations of what was to come later in the series. The number of times any particular anticipation error was made seemed to be a function of its remoteness from the correct response. He found an inverse curvilinear relation between the degree of anticipation and the frequency of the anticipatory errors. He found no evidence for a similar backward association.[7]

In analyzing the errors made by 11 Ss in memorizing lists of 10 three-place numbers by the *anticipation* method, Mitchell found that the number just beyond the correct response was given in whole or part much more frequently than could

TABLE I

ANTICIPATION OF NUMBERS MADE BY 11 Ss IN LEARNING 552 LISTS

	Degree of Anticipation										
	1	2	3	4	5	6	7	8	9	10	Total
No. of Responses	23,396	478	77	48	48	44	56	25	15	9	24,196
% of 800 Antic. Errors		59.8	9.6	6.0	6.0	5.5	7.0	3.3	1.9	1.1	

be attributed merely to chance.[8] Due to the limited number of digits and their frequent repetition within a list, it was impossible to analyze the errors as parts of more remote numbers. Nevertheless, when a whole three-digit response was made that was identical with some number in the list, it was tabulated as an error of "Wrong Position in List."[9] There were 800 of these, at least 799 of which may be considered real anticipations and not merely chance, guesses, for the chances of guessing a three-place number correctly are only about one in a thousand. For this reason, this material is peculiarly well adapted for studying anticipations. In the maze material used by Lumley—and even in his two-place number series—the repetitions within the lists and the small number of possible responses make the chance anticipations so numerous that it is very difficult to segregate the real anticipations.

Out of the 800 anticipatory errors found by Mitchell, 478 or 60% were the

[5] *Op. cit.*, 76.

[6] F. H. Lumley, An investigation of the responses made in learning a multiple-choice maze, *Psychol. Monog.*, 42, 1931, (no. 189), 26; Anticipation of correct responses as a source of error in the learning of serial responses, *J. Exper. Psychol.*, 15, 1932, 204.

[7] *Op. cit., Psychol. Monog.*, 42, 1931, (no. 189), 28; cf. also Anticipation as a factor in serial and maze learning, *J. Exper. Psychol.*, 15, 1932, 333-334.

[8] M. B. Mitchell, Errors in the memorization of numbers, this JOURNAL, 45, 1933, 1-16.

[9] *Op. cit.*, 11 ff.

next number in the list beyond the correct response; in other words, 60% of these errors were anticipations of the second degree. This part of the present report is primarily an analysis of the remaining 322 responses which were numbers more remote in the lists. However, for completeness the correct responses, or those whose degree of anticipation might be said to be one, are also included in Table I. Since these compose 96.7% of the total number of anticipatory responses, the percentages for all the other 9 degrees of anticipation become comparatively too small

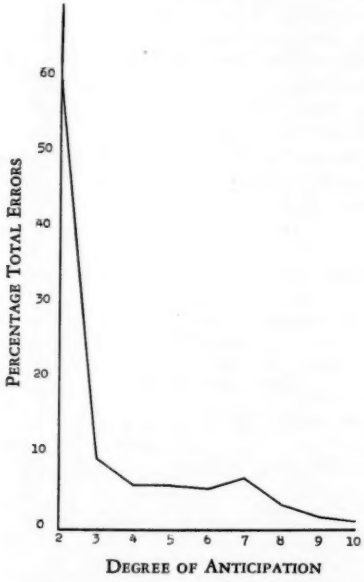

FIG. 1. ANTICIPATION ERRORS MADE BY 11 *Ss* IN LEARNING 552 LISTS

to be distinguished graphically. Consequently, the correct responses are omitted from the graph. The percentages of the total 800 errors which each degree of anticipation furnishes are included in Table I and shown graphically in Fig. 1. Thus remote numbers do appear as a source of error in the memorization of lists of numbers, at least under the conditions of this experiment. The frequency of the appearance of a number in the list as an error seems to be dependent upon its distance from the correct number. In general, the more remote the number, the less frequently it appears as an error. The fluctuations seen in the graph are probably due to sampling errors.

The results of this experimentation give no evidence of backward associations. There was no time interval elapsing between the end of one trial and the beginning of the next; the last number of the list was the cue to give the first number

in the list. Since the numbers were shown in a continuous series with no extra time between trials, any forward association could just as well be considered a backward association. For instance, the ninth degree of forward association would be the same as a backward association of the first degree, and the eighth degree of forward association would be the same as a backward association of the second degree. From the results of Ebbinghaus,[10] one would expect these backward associations of the first and second degree to be greater than the remote forward associations of the sixth and seventh degree. This would cause a rise at the end of the curve, making it somewhat U-shaped. This, however, is not the case; the eighth and ninth degrees of anticipation are smaller than the sixth and seventh degrees. The curve does not rise at the right hand side to indicate the presence of any backward associations—at least, as far remote as the sixth and seventh degree.

The results presented here are essentially in agreement with the remote forward associations found by Ebbinghaus, and the anticipation found by Lumley in the errors made by his *Ss*. It also confirms Hall's finding of remote associations formed during serial learning although he demonstrated their functioning after learning rather than during the learning. The seemingly different results of Cason are probably due to the relative meaningfulness of most of his original material as compared with his test material and the material used by other experimenters. The technique used in this work and in Lumley's does not involve a frustration of associative tendencies as does the technique of Ebbinghaus, Hall, and Cason in breaking up the material for relearning.

Part II

In Part I data are presented to indicate the presence of forward associations in the serial memorizations of numbers by the anticipation method. These were shown to exist by the presence of anticipatory place-skipping errors. In Part II such questions are asked as: At what stage of the learning process do they occur in the learning of the list? Where in the list do they occur? and Whence do they come? *i.e.* where should they occur in the list? These facts are of some significance in view of the theories of serial learning recently set forth by Lumley[11] and Lepley,[12] and considered in Part III.

The question might first be asked as to when these anticipatory or place-skipping errors occur. Are they most frequent when *S* is first beginning to memorize the list, when it is nearly learned, or somewhere in between? By taking all lists with a given number of trials, it was possible to find the number of place-skipping errors during each trial. For instance, 64 lists were found that required exactly 8 trials for learning. In the learning of these lists, 81 place-skipping errors were made; 24 of these were made in the fifth trial, while only 1 occurred in the first trial and none in the eighth. Similarly, when all the lists with 10 trials were analyzed, it was found that the greatest number of place-skipping errors was found in the fourth and sixth trials with 10 each, out of the total of 62. The first trial had

[10] Ebbinghaus, *op. cit.*, 151-156.

[11] F. H. Lumley, Anticipation as a factor in serial and maze learning, *J. Exper. Psychol.*, 15, 1932, 341-342.

[12] W. M. Lepley, A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 39, 1932, 283-284.

only 2 and the last trial none, while the intermediate trials at the beginning of the list steadily increased and those beyond the middle decreased in number of place-skipping errors. The anticipatory place-skipping errors, therefore, seemed to occur most frequently, not near the beginning nor the end of the learning, but in the middle. The number of trials needed in the learning of any list ranged from 3 to 40, so that the number of errors falling under any of these classifications was very small. Even the number of trials occurring most frequently (described above) had only 81 errors to be distributed among 8 trials. Therefore, in this manner, only a rough indication could be obtained of the location of the greater number of these errors.

To obtain more accurate information, the results for all lists were pooled. The learning time was divided into quarters, and the number of place-skipping errors made in each quarter for all lists was tabulated. (See Table II.) Practically the same number of place-skipping errors was found in the second and third quarters of the learning time, and together these two quarters contained two-thirds of all these errors. Hence the errors were most frequent in the middle of the learning.

In analyzing the errors made by these *Ss*, it was often found that there were differences between the *Ss* who learned rapidly and those who learned more slowly.[13]

TABLE II

PLACE-SKIPPING ERRORS MADE IN EACH QUARTER OF THE LEARNING TIME FOR ALL LISTS

Group	Errors	Quarter				Total
		1	2	3	4	
Slow	No.	68	95	103	56	322
(5)	%	21	30	32	17	
Fast	No.	72	167	154	73	466
(5)	%	15	36	33	16	
Total	No.	141	265	261	133	800
(11)	%	18	33	33	17	

In the first place, although the time for learning was much greater for the slow group than for the fast group (as was also the total number of errors), the number of place-skipping errors was considerably greater for the fast group. This is in accordance with the theory set forth in that article, that the faster *Ss* were learning the numbers as wholes.[14] The fast group had 69% of their errors in the two middle quarters, with about the same number in each. The slow group also had more in these two quarters than in the other two, but the difference was not so great; only 62% were found in the middle. The discrepancy between the two groups is probably explained by the fact that as the number of trials decreased, the absence of place-skipping errors in the last trial, and the very small number in the first trial, became relatively more important in determining the per cent of the total number of such errors in the first and last quarters. For instance, the first trial was the whole first quarter, when only four trials were given, while it was only one-tenth of the first quarter when 40 trials were given. There is, therefore, some evidence that the

[13] Mitchell, *op. cit.*, 6 ff.
[14] *Op. cit.*, 7.

place-skipping errors are more frequent in the middle of the learning time than at the beginning or near the end.

Each number that is a place-skipping error has two aspects. It is on occasion a stimulus and a response. It is a stimulus because it is on the drum once during each trial while *S* tries to give the next number in the list. It is also a response to a stimulus, the stimulus being the number exposed on the drum when it is given. Table III is a summary table giving both aspects of the errors for each quarter of

TABLE III

PLACE-SKIPPING ERRORS FOR EACH QUARTER OF LEARNING TIME

	Quarter	Errors	Position in List										Total
			1	2	3	4	5	6	7	8	9	10	
Stimulus Aspect	1	No.	73	5	9	8	8	6	11	5	7	9	141
		%	52	4	6	6	6	4	8	4	5	6	
	2	No.	26	11	15	27	24	26	43	23	40	30	265
		%	10	4	6	10	9	10	16	9	15	11	
	3	No.	3	8	14	31	26	36	42	44	39	18	261
		%	1	3	5	12	10	14	16	17	15	7	
	4	No.	1	2	11	8	23	7	29	21	21	10	133
		%	1	2	8	6	17	5	22	16	16	8	
	Total	No.	103	26	49	74	81	75	125	93	107	67	800
		%	13	3	6	9	10	9	16	12	13	8	
Response Aspect	1	No.	6	10	7	13	6	8	6	8	11	66	141
		%	4	7	5	9	4	6	4	6	8	47	
	2	No.	6	16	26	34	34	32	29	39	25	24	265
		%	2	6	10	13	13	12	10	15	9	9	
	3	No.	6	15	23	28	40	35	42	36	24	12	261
		%	2	6	9	11	15	13	16	14	9	5	
	4	No.	2	5	4	21	12	26	20	30	10	3	133
		%	2	4	3	16	9	20	15	23	8	2	
	Total	No.	20	46	60	96	92	101	97	113	70	105	800
		%	3	6	8	12	12	13	12	14	9	13	

the learning time and for the whole time. If we consider the stimulus aspect of these errors, *i.e.* their true position or position where they should have occurred within the list, we see that taking the whole learning time, the errors most frequently come from the seventh, ninth, first, and eighth positions respectively. The reason for the frequency of the first position is that the *S*s usually learned the first number of the list most easily.[15] They often gave it in the tenth position, especially during the first quarter (frequently even during the first trial), thereby giving it

[15] Mitchell, The effect of serial position in continuous memorization of numbers, this JOURNAL, 45, 1933, 493-494.

one place too soon and making the most frequent type of anticipation error, that of skipping one place. From verbal reports given by the *Ss*, it was evident they thought it was time to begin the second trial. Some of the *Ss* resorted to quite overt methods of counting the numbers in order to avoid such errors at the beginning. Even then, during the first quarter of the learning time, the number of these errors, which were numbers belonging in the first position in the series, far exceeds those from any other position in the list. During the second, third, and fourth quarters, the numbers from the later positions in the list were more frequent sources of error. The numbers from the first position were frequent enough, however, to rank third place for the entire learning time.

When we consider the response aspect, the position in which the errors occurred, we see that during the second, third, and fourth quarters of the learning time, the fourth through the eighth positions contained the most place-skipping errors. During the first quarter, the tenth position contained the most place-skipping errors. This has been explained above; it contained the numbers from the first position in the list. They were frequent enough to have the errors in the tenth position rank second place in number of errors for the entire time.

We have seen whence the place-skipping errors came. With the exception of the first position in the list, they came from positions near the end of the list which were learned last.[16] Were these place-skipping errors made before the numbers were learned with their position? This question may be considered statistically from two points of view. In the first place, how frequently was the number given as an error before it had ever been given in its correct position? Of all the place-skipping errors, 385 or 48% occurred before the number had been given in its true position at least once. Thus just about as many place-skipping errors occurred before as after the number had been given correctly, at least once. This was also true when the 5 *Ss* who learned the slowest were compared with the 5 who learned the fastest. In the second place, how frequently was the number given as an error after it had been completely learned as judged by the fact that it was given correctly in each succeeding trial? Of all these errors, 556 or 69.5% were made before the number was completely learned in its true position. Thus in the majority of cases, the place-skipping errors had their source in numbers not yet completely learned in their true position, but 30% of them did occur after the number had been learned. Since 48% of the errors occurred before the number was ever given correctly in its true position, and approximately 70% of the errors occurred before the number was completely learned, the difference between these per cents or 22% of the errors might be said to have occurred somewhere during the process of learning the number in its true position.

Summarizing the results, then, the place-skipping errors were shown in Part I to be inversely related to the remoteness from the true position. In Part II, they were shown to be most frequent in the second and third quarters of the learning time. The latter positions in the list (not including the last) were the most frequent location of both the true positions of the numbers in the list and the positions where the place-skipping errors occurred. The only exception was the number from the first position which was frequently given in the tenth position. The place-skipping errors usually occurred before the number was completely learned in its true position, but 30% of the errors occurred after the number had been learned.

[16] *Op. cit.*, 493.

PART III

The foregoing results raise some theoretical considerations. The presence of remote anticipatory tendencies has great biological significance. As Hull has pointed out, there is a tendency, "where the conditioned stimulus precedes the unconditioned one in the conditioning process, for the reaction to be attracted forward toward the former. Under certain circumstances the reaction (after a number of reënforcements) may begin a considerable interval before the delivery of the unconditioned stimulus. In the case of certain defense reactions this may even result in the organism not receiving the nocuous unconditioned stimulus at all."[17] In the serial memorization of numbers, however, this tendency for the responses to be given too early proves undesirable and must be inhibited. The analogous process in conditioning reflexes Pavlov[18] calls the *inhibition of delay.*

Lumley found that in learning letter mazes, the *S*s grouped the material, usually in verbal groups.[19] In serial learning of various kinds, *S*s have grouped the material either arbitrarily or in some especially suitable manner. Lumley concludes that in serial learning, the *S*s first made rough localizations as near the beginning, near the middle, and near the end.[20] As the learning progresses, they localize more exactly, so that as the classifications contain fewer units, the relative frequency of anticipating the nearer numbers becomes greater and greater. In other words, the ratio of the more remote anticipations to the near anticipations becomes less as the learning progresses.[21] On this hypothesis, it would seem, that *S* would be just as apt to make a response preceding the true response as one following the true response. For instance, if *S* has a rough localization of 6 letters as "near the beginning," then in trying to recall the fourth letter, he might give one of the first three as well as the fifth or sixth. Lumley failed to find any evidence for such backward associations. This lack of evidence seems rather damaging to his theory. The forward associations, however, were more favorable to the theory. In a later article,[22] he summarizes the results of experiments using letter mazes, two-place numbers, a paper maze, and a foot maze, all of which give rather convincing evidence of this change as the learning progresses.

If this is true for all serial learning, then it would seem that an analysis of the 800 place-skipping errors in this report should show a larger ratio for the remote anticipations to the near anticipations in the early part of the learning than in the latter part. Table IV gives a summary of all the place-skipping errors for each degree of anticipation. It also gives the percentage of the total number of errors for each quarter for each degree of anticipation. For instance, while there are only 98 place-skipping errors of the second degree of anticipation in the first quarter, that is 70% of the total place-skipping errors for the quarter. In the second quarter,

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[17] C. L. Hull, A functional interpretation of the conditioned reflex, *Psychol. Rev.*, 36, 1929, 499.

[18] I. P. Pavlov, *Conditioned Reflexes,* 1927, 41.

[19] F. H. Lumley, An investigation of the responses made in learning a multiple choice maze, *Psychol. Monog.*, 42, 1931, (no. 189), 20.

[20] *Op. cit.*, 28; Anticipation of correct responses as a source of error in the learning of serial responses, *J. Exper. Psychol.*, 15, 1932, 196-197.

[21] *Op. cit., Psychol. Monog.*, 42, 1931, (no. 189), 28; *J. Exper. Psychol.*, 15, 1932, 205.

[22] Lumley, Anticipation as a factor in serial and maze learning, *J. Exper. Psychol.*, 15, 1932, 342.

there are 146 place-skipping errors of the second degree of anticipation, but this is only 55% of the total for the second quarter. The third and fourth quarters each have 59% of the errors classified as errors of the second degree. It is evident, therefore, that the total number of second degree anticipations does not show the expected increase by quarters of the learning time. Nor do the remaining degrees of anticipation show a consistent increase for the smaller degrees of anticipation and

TABLE IV

NUMBER AND PERCENTAGE OF PLACE-SKIPPING ERRORS FOR EVERY DEGREE OF ANTICIPATION BY QUARTERS OF LEARNING TIME

	Quarter	Errors	Degree of Anticipation 2	3	4	5	6	7	8	9	10	Total	Ratio
All Errors	1	No.	98	16	8	1	4	7	3	1	3	141	.11
		%	70	11	6	1	3	5	2	1	2		
	2	No.	146	25	16	19	18	22	9	7	3	265	.20
		%	55	9	6	7	7	8	3	3	1		
	3	No.	155	20	14	21	13	20	9	7	2	261	.18
		%	59	8	5	8	5	8	3	3	1		
	4	No.	79	16	10	7	9	7	4	0	1	133	.11
		%	59	12	8	6	7	6	3	0	1		
	Total	No.	478	77	48	48	44	56	25	15	9	800	
		%	60	10	6	6	6	7	3	2	1		
Second Degree Errors Occurring in Tenth Position Omitted	1	No.	34	16	8	1	4	7	3	1	3	77	.24
		%	44	21	10	1	5	9	4	1	4		
	2	No.	128	25	16	19	18	22	9	7	3	247	.22
		%	52	10	6	8	7	9	4	3	1		
	3	No.	154	20	14	21	13	20	9	7	2	260	.18
		%	59	8	5	8	5	8	3	3	1		
	4	No.	79	16	10	7	9	7	4	0	1	133	.11
		%	59	12	8	6	7	6	3	0	1		
	Total	No.	395	77	48	48	44	56	25	15	9	717	
		%	55	11	7	7	6	8	3	2	1		

decrease for the larger degrees of anticipation. When the ratios for the last four degrees of anticipation (7, 8, 9, and 10) to the first four (2, 3, 4, and 5) are figured for each quarter, there is a rise from the first to the second quarter and a drop from the third to the fourth quarter. This corresponds exactly with the gross number of place-skipping errors found in each quarter. This also fits Lepley's "alternate conception of the nature of the excitatory and inhibitory phases of these remote tendencies."[23]

It was pointed out in Part II of this article, that many of the second degree anticipations occurring in the first quarter were located in the tenth position of the

[23] W. M. Lepley, A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 39, 1932, 283.

list and were due to the fact that the *S*s thought it was time for the second trial to begin. There was no break between trials; the tenth number was the cue for the first number. The apparent discrepancy, therefore, between these results and those of Lumley, might be due to this continuous presentation of the material for memorization. In all of Lumley's experiments, the trials were quite distinct from the beginning and did not permit this type of error in the first trial. For this reason, the results have been retabulated (lower half, Table IV) omitting all the place-skipping errors of the second degree of anticipation occurring in the tenth or last position of the list. This reduced the percentage of second degree errors in the first quarter from 70 to 44. There is now an increase in the percentages of second degree anticipations from the first quarter to the third and, at least, no drop for the fourth quarter. This suggests that when the errors of the second degree occurring in the tenth position are omitted, the results agree with those of Lumley. In other words, when the data are considered as discontinuous learning, the more remote anticipations seem to decrease and the nearer anticipations to increase with practice. When the data were treated as continuous this phenomenon was not apparent, at least not in the first quarter of the learning.

Although Lumley's reports and this one all deal with serial learning, there are many differences. For the most part, he used mazes of various kinds, in which the number of choices at any one time was so limited that it was quite impossible to make any response that could not be considered an anticipation of a true response to follow. In maze learning *S* had to keep on trying after one error until he made the correct response. He was not shown the correct response after one error as he was in learning a list of numbers. This lack of punishment, as it were, in learning numbers might have made a difference in the results. Even Lumley's serial number learning was quite different from that described in this article. He used two-place numbers instead of three-place numbers. It was, therefore, much easier for his *S*s to anticipate the number correctly by mere chance. This report considers only the anticipations in which all three digits were given correctly and in their proper order. This accounts for the fact that the place-skipping errors were only about 2% of the total number of errors made by the *S*s. The errors in which only one or two digits were given also showed anticipation. For the second degree of anticipation this was much larger than could be attributed to chance.[24] Due to the small number of digits in our number system, there were so many repetitions in a series of three-place numbers, that it was impossible to consider the source of these responses beyond the second degree. Even in a letter maze, Lumley found it advisable to eliminate some errors from his totals because of this factor of repetition in the material.

With the exception of the foot-maze, Lumley did not use quarters of learning time in handling his material as was done in preparing Tables III and IV. He used rather arbitrarily chosen sections. "The first section contains all the errors made by the subject in the first trial. Here, as one would expect, there is no relation between the amount of anticipations and the number of units skipped. . . . The errors made on the second and subsequent trials were divided into the three sections as evenly as possible, and each point of the maze was considered separately to neutralize the effect of the preponderance of errors near the center of the maze."[25]

[24] Mitchell, Errors in the memorization of numbers, this JOURNAL, 45, 1933, 8-16.
[25] Lumley, *op. cit., Psychol. Monog.*, 42, 1931, (no. 189), 23.

In an attempt to make the results of the present experiment more comparable with those of Lumley, the 800 place-skipping errors were divided into sections such as those just described (Table V). Since the *S*s in this experiment were not encouraged to guess during the first trial (as Lumley's were), but were merely told to study the numbers as they appeared, the errors in Section I consist primarily of the oft mentioned errors of the second degree of anticipation occurring in the tenth position. The errors in Sections II, III, and IV each consist of the place-skipping errors occurring in one-third of the errors made in learning any particular number.

TABLE V

PLACE-SKIPPING ERRORS BY SECTIONS FOR EACH DEGREE OF ANTICIPATION

Section		Degree of Anticipation									Total	Ratio
		2	3	4	5	6	7	8	9	10		
I	No.	18	1	1		2					22	0
	% 22*	82	5	5		9						
	% 1752†	.24	.01	.01		.03						
II	No.	109	14	10	2	7	9	2	2	3	158	.12
	% 158*	69	9	13	2	4	6	2	2	2		
	% 9447†	1.15	.15	.11	.02	.07	.10	.02	.02	.03		
III	No.	130	24	13	19	13	21	12	4	4	240	.22
	% 240*	54	10	5	8	5	9	5	2	2		
	% 9447†	1.38	.25	.14	.20	.14	.22	.13	.04	.04		
IV	No.	228	36	20	25	26	25	13	6	1	380	.15
	% 380*	60	9	5	7	7	7	3	2	.3		
	% 9447†	2.41	.38	.21	.26	.28	.26	.14	.06	.01		

* Percentage of the total number of place-skipping errors for the section.

† Percentage of the total numer of errors of all kinds for the section. "Ratio" is the sum of the place-skipping errors having a degree of anticipation equal to 7, 8, 9 and 10 divided by the sum of the space-skipping errors having a degree of anticipation equal to 2, 3, 4, and 5. Section I consists of all the errors in the first trial of the learning of any number. Sections II, III, and IV each consist of one-third of all the remaining errors made in the learning of each number.

For instance, if 6 errors were made after the first trial in learning a particular number and the second error was a place-skipping error, it would go into Section II no matter in what quarter of the learning time it occurred. Similarly, if the sixth error was a place-skipping error, it would go into Section IV, even if it occurred in the first quarter of the learning time.

Table V gives the number of place-skipping errors in each section for each degree of anticipation. There is a consistent increase in number of place-skipping errors from Section I to Section IV for every degree of anticipation except the tenth.[26] This is not surprising when it is considered that all three-digit responses increased in number as the learning progressed.[27] The place-skipping errors, as a part of these three-place responses, might be expected to increase. In Part II of this article,

[26] Since the tenth degree of anticipation contains very few errors and amounts to reading off the number exposed on the drum, it is quite insignificant.

[27] Mitchell, Errors in the memorization of numbers, Doctorate dissertation on file in the Yale University Library, Appendix E.

they were shown to be most numerous in the second and third quarters. The percentage of the total number of all errors for the section is given in Table V. The results indicate two things: first, they show the increase from Section I to Section IV corresponding to the increase in number of place-skipping errors; and secondly, they show what a small per cent they really are of the total number of errors. The largest number of errors for any degree of anticipation is 228 for the second degree in Section IV, and yet it is only about 2% of all the errors in the section. The percentages of the place-skipping errors in each section for each degree of anticipation do not show a consistent rise from Section I to Section IV.

The ratio of the last four degrees of anticipation to the first four degrees of anticipation do not show a decrease as the learning progresses. The ratios are 0, 0.12, 0.22, and 0.15. They do not seem to indicate a relative increase in near anticipations and decrease in remote anticipations. It was thought that when the second degree anticipations occurring in the tenth position were eliminated from these sections of learning the same results might be obtained as were apparently found when the quarters of learning time were thus treated. However, the ratios were changed to 0, 0.19, 0.24, and 0.15, which no more show a steady decrease from Section I to IV than those previously obtained using all the errors.

Evidence supporting Lumley's theory, therefore, can hardly be said to be obtained from place-skipping errors occurring in the memorization of three-place numbers. As a particular number is learned, there is an increase in place-skipping errors, but the increase in anticipation is distributed throughout all degrees of remoteness and not confined to the lesser degrees of anticipation. This study, however, is restricted to the errors containing a complete three-digit number. It does not include the anticipations of one- and two-digits alone. If it were possible to calculate them, they might well show a relative increase in near anticipations and decrease in remote anticipations as the numbers were learned.

Summary

In learning lists of 10 three-place numbers by the anticipation method, 800 place-skipping errors were made.

(1) On the whole, the number of the errors for each degree of anticipation was an inverse curvilinear function of its distance from the true position in the list.

(2) No evidence was found for backward associations.

(3) The place-skipping errors were most frequent in the second and third quarters of the learning time as would be expected since at the beginning the *S*s would not know the numbers to give them in the wrong position, and at the end they would only give them in their correct positions.

(4) Most of the place-skipping errors occurred in the serial positions just beyond the middle of the list and were numbers with true positions also located there.

(5) Usually these errors occurred before the numbers had been completely learned in their true position, but, paradoxically, 30% of them occurred after the numbers had been learned.

(6) These data were quite unsatisfactory in giving evidence for Lumley's theory that the more remote degrees of anticipation decrease and the nearer ones increase as the learning progresses. There was instead a relative rise and then decline of remote anticipations.

(7) In learning a particular number, the place-skipping errors of most all degrees tend to increase as the learning proceeds.

THE RELATION OF AFFECTIVE TONE TO THE LEARNING AND RECALL OF WORDS

By M. M. WHITE and MARGARET MCLEOD RATLIFF, University of Kentucky

Numerous experiments have been performed to determine the memory value of pleasant and unpleasant words. Of these Meltzer has summarized twenty-six.[1] The results of these experiments are markedly inconsistent. Such lack of agreement indicates that more experiments should be performed in order to establish the facts and if possible to explain the divergent conclusions of previously reported experiments.

EXPERIMENT I

Procedure. A list of 240 words, most of which were taken from the Pressey X-0 test, was given 150 students in beginning psychology with instructions to mark every word either as very pleasant, pleasant, indifferent, unpleasant, or very unpleasant. The students were urged to make their judgments quickly and not to revise any rating. Two weeks later the same students were again given this list of words and instructions. No reference was made to the fact that the words were the same on both occasions. The order of the words was reversed on the second presentation.

From the list of 240 words, 10 pleasant (P), 10 unpleasant (U), and 10 indifferent (I) words were selected. Pleasant and unpleasant words were those on which we found (1) agreement among all *Ss*, (2) agreement between the first and second ratings, and (3) at least one superlative rating by every *S* on one of the two occasions. Since it was impossible to select 10 indifferent words on the basis of these criteria, those words were chosen which most nearly satisfied these conditions. Because the inconsistency of marking makes the classification doubtful, no results are given concerning the indifferent words.

Table I shows the words used in all experiments to be reported. In one experiment no indifferent words were used.

Six different arrangements of the words were designed to offset the advantage of position in the series. In these arrangements the first, second, last, and second last positions were filled twice by P, twice by U, and twice by I words. Intermediate positions were filled by the P, U and I words in a random order which remained the same in all arrangements.

Of the 150 *Ss* who had rated the words, 42 volunteered as *Ss* for the second part of the experiment. They were arbitrarily divided into 6 groups, each group containing 4 men and 3 women. Every *S* in a group was individually presented with one of the 6 arrangements of words described above, by means of the Ranschburg memory apparatus. Instructions to the *Ss* were as follows.

* Accepted for publication Oct. 31, 1932.

[1] H. Meltzer, The present status of experimental studies of the relationship of feeling to memory, *Psychol. Rev.*, 37, 1930, 127-139.

"I am going to show you 30 words which you are to try to remember. Each will be shown for about 2 sec., and the whole list of words will be presented until you are able to anticipate correctly all the words in the order in which they are shown: that is, you are expected to say aloud the word following the word being shown on the apparatus. Immediately preceding the presentation of the words, you will see numbers 1, 2, and 3. After the first presentation of the entire list, you will be expected upon seeing number 3 to say the first word; upon seeing the first word, to say the second word; and so on through the series until the entire list has been learned. I am going to keep a record of your responses; so speak clearly."

TABLE I

Pleasant	Indifferent	Unpleasant
flower	staple	fever
girl	cover	poison
laugh	ear	insult
sweet	floor	dead
father	bucket	disgrace
lover	answer	vomit
smile	inkstand	terror
beauty	paper	funeral
home	pedal	fear
eat	cotton	insanity

After each presentation 30 sec. elapsed before the words were again shown. Presentations were repeated until *S* correctly anticipated all the words on one trial, or until 30 presentations had been given. Nineteen *Ss* anticipated all the words on one trial within the 30 presentations; 23 did not.

E calculated from his record: (1) the number of the trial on which each particular word was first recalled regardless of the correctness of the anticipation; (2) the number of the trial on which a particular word was correctly anticipated; and (3) the number of the trial after which *S* always correctly anticipated the word.

TABLE II

NUMBER OF REPETITIONS REQUIRED FOR REPRODUCTION OF WORDS

Av. trials	First Anticipation regardless of order		First Anticipation in correct position		Beginning of consistent anticipation	
	P	U	P	U	P	U
Entire group	8.99	8.43	11.03	10.61	17.64	17.70
(S.E. Diff.)	(7.15)		(8.48)		(14.04)	
19 Ss	7.14	6.92	8.15	8.05	12.08	12.50
23 Ss	10.52	9.68	13.42	12.72	22.23	22.00
18 women	8.16	7.62	9.83	9.46	15.66	15.90
24 men	9.61	9.04	11.93	11.47	19.12	19.06

Half of the *Ss* were asked to return to the laboratory after one, the other half after two weeks. They were not told for what purpose the request was made. On their return each was asked to write all the words remembered.

Results. The results are presented in Table II. The first line shows the results for the group as a whole in terms of the average number of trials required correctly to anticipate the words. The second line shows the same thing for the 19 *Ss* who

succeeded in correctly anticipating all words on one trial within 30 presentations. The third line shows the results for all the words correctly anticipated by the 23 *Ss* who were unable within 30 presentations to complete the learning task. The fourth and fifth lines present the data distributed according to sex.

The standard error of the difference for the group as a whole indicates that the differences in learning between the P and the U words are negligible.

Attention is called to the trends in the table as they relate to the hypothesis of active forgetting of U words, which we shall find is supported by the experiments reported below. While the U words on the average required fewer repetitions for initial recall for the group as a whole, the consistent recall in correct position began on the average earlier for the P words. The earlier consistent recall for the

TABLE III

COMPARISON OF NUMBER OF WORDS RECALLED BY 40 *Ss* ACCORDING TO ENTIRE GROUP, SEXES, AND INTERVAL OF DELAY*

Period of recall	Av. no. words recalled		
	P	U	Diff.
End of learning period (40 *Ss*)	9.35	9.54	− .19
7 days later (18 *Ss*)	8.11	7.94	+ .17
14 days later (22 *Ss*)	6.27	4.81	+1.44
End of learning period (18 women)	8.55	8.72	−.17
7 days later (10 women)	8.70	8.50	+.20
14 days later (8 women)	7.50	6.00	+1.50
End of learning period (22 men)	8.09	8.23	−.14
7 days later (8 men)	7.62	7.00	+.62
14 days later (14 men)	5.57	4.14	+1.43

* Two of the 42 *Ss* were unavailable for recall.

P words holds not only for the group as a whole, but for the 19 *Ss* who completed the learning task. When the correctness of position of the recalled word is ignored, however, both sexes required fewer trials to recall the U than the P words.

As a whole, nevertheless, Table II shows little or no difference between the average number of trials required to recall P and U words, if the recall is measured in terms of the 3 criteria enumerated above.

Table III shows the data for immediate and delayed recall. Although more U than P words were recalled immediately (9.5 U and 9.3 P), more P than U words were recalled at the end of 7 and 14 days. Furthermore, the preponderance of P over U words is greater after the longer period (a difference of 1.44 words after 14 days as compared to 0.17 words after 7 days).

How are we to explain this difference between immediate and delayed recall? The results, while in accord with the hypothesis of active forgetting, should be considered as little more than suggestive. We therefore made Experiments II and III, which follow immediately, in an attempt to test more thoroughly this hypothesis.

EXPERIMENT II

Procedure. Experiment I was repeated with the following changes. (1) The words were presented a given number of times and not until mastery. (2) The record was kept in terms of the number of words recalled. (3) Longer time intervals were used between immediate and delayed recall. (4) No I words were used. It is to be noted that the criterion of learning used was that which in the previous experiments yielded readier learning for the U words.

The *Ss*, 25 in number, were not taken from the 150 students who had rated the words. We felt, however, that since all of the 150 students had agreed in their marking of the words used, the chances were that these 25 *Ss*, also taken from the psychology classes, would give similar judgments of pleasantness and unpleasantness. To check this supposition, after the experiment was finished the *Ss* were asked to mark a list of 60 words which contained the stimulus-words used. That the difficulty which the *Ss* had experienced in recalling the words did not determine their ratings is indicated by the facts that (1) whereas only 20 of the 60 words were used in the learning, these students' ratings on the entire list of 60 agreed with the ratings of the 150 students; (2) the alleged P words which had been forgotten were not marked U.

TABLE IV

NUMBER OF PLEASANT WORDS RECALLED IN TERMS OF THE PERCENTAGE OF TOTAL WORDS RECALLED

	Interval in days				
	0	7	14	21	28
No. *Ss*	25	7	6	4	7
% P words recalled	52	62	73	72	72

Each of the 20 stimulus-words was typed on a 3 x 5 in. index card and presented in a different random order to each *S*. Each card was exposed for approximately 2 sec. The entire list was presented 5 times. The instructions to the *Ss* were

"I am going to show you a list of 20 words which you are to try to remember. After you have seen the list 5 times I will ask you to write as many of the words as you remember."

Delayed recall was taken for 7 *Ss* after one week, for 6 after 2 weeks, for 4 after 3 weeks, and for 7 after 4 weeks. One *S* failed to report at the proper time. The *Ss* were not told that there would be a delayed recall.

Results. Table IV gives the P words recalled in terms of the percentage of the total number of words recalled. It will be noted that only 52% of the total words recalled immediately were P, whereas this percentage had increased to 73% after a delay of 14 days. After the 14-day interval there is a change of only 1% in the number of P words recalled.

Table V gives the number of *Ss* that recalled more P than U words for the various periods of delay: 0, 7, 14, 21, and 28 days. It also gives the data for those who recalled more U than P words, and for those who recalled an equal number of P and U words. If we consider the *Ss*, we find, as is to be expected from Table IV, that the number of *Ss* who recalled more P than U words increases through the delay of 14 days and not afterwards. Table V shows the same general

trend as Table IV. We may conclude that regardless of whether one considers *Ss* or words, there is an increase in the preponderance of P words through the 14-day delay and not afterwards.

In the first experiment, combining all criteria of learning, we found no difference in immediate recall between P and U words. In this experiment a slight tendency for the P words to be remembered better was revealed. This apparent contradiction may be due to the fact that in one experiment the *Ss* were instructed to master,

TABLE V

Ss' RECALLS COMPARED

	Interval in days				
	0	7	14	21	28
No. Ss recalling more P words	12	5	6	4	6
No. Ss recalling more U words	8	1	0	0	1
No. Ss recalling same no. of each	5	1	0	0	0

whereas in the other they were not given such instructions and none of them succeeded in mastering the series. If this reason is correct, we may have an explanation of the inconsistent results of the experiments on affectively toned words. To check this suggestion a third experiment was performed.

EXPERIMENT III

Procedure. Two lists of 15 words each were read once to 239 students in beginning psychology, with instructions to learn. Presenting the words only once made complete mastery even less possible than in Experiment II. The purpose of this further reduction in presentations was to determine more fully the effect of partial learning on the forgetting of pleasant and unpleasant words. The students were taken in groups of approximately 20 each. One of the lists began and ended

TABLE VI

COMPARISON OF THE WORDS RECALLED BY 239 *Ss*

Relation of P and U words	No. Ss giving relation	Words recalled: P Av.	P σ	U Av.	U σ
P>U	135	6.48	1.17	5.85	1.18
P<U	65			Diff. = 0.63	
P=U	39			σdiff. = 0.11	

with P words; the other with U words. Each list contained 5 U, 5 P and 5 I words. The affectively toned words were those used in Experiments I and II.

The students were asked to write all the words remembered immediately after the experimenter finished reading the list.

Results. The results are shown in Table VI. The table shows that the majority of *Ss* remembered more P than U words (135 compared to 65). The table also shows that more P than U words were remembered and that the difference in recall

is statistically reliable, being 5 times its σ. It seems that if complete mastery is not required more P than U words are recalled. We cannot be certain of this conclusion, however, for in the first experiment, where instructions to master were used, the words were presented visually. Perhaps auditory presentation made the P words easier than the U words, though this seems improbable, especially since in Experiment II where visual presentation was used and the instructions were the same as in Experiment III, more P than U words were recalled.

Discussion

The results of these three experiments support a theory of active forgetting. In the first experiment, practically no difference was found between P and U words in the ease of learning. In the other two experiments a difference was found. Since the first experiment was the only one that contained instructions to master, and approximately 50% of the *Ss* did master, we believe that these instructions caused the difference in the results. Perhaps the mechanism which accounts for active forgetting of the unpleasant also causes the unpleasant to be accepted more quickly than the pleasant when once the organism realizes that the unpleasant is inevitable. This suggestion is borne out by the fact that in Experiment I, the U words were recalled earlier than the P words if the correctness of the position was ignored.

Several important points in these experiments should be emphasized. (1) Every *S* in Experiment I had judged the affective tone of the words on two different occasions. We are therefore safe in assuming that the material used was affectively toned for our *Ss*—an assumption which is made but not justified in several of the experiments summarized by Meltzer. (2) In Experiment I the P and U words were of practically the same difficulty in immediate learning, as evidenced by three related criteria. (3) More P than U words were remembered in delayed recall in every experiment. (4) Roughly, the longer the time interval, the greater the superiority of the P words over the U (Experiments I and II). (5) If the *Ss* are instructed to master completely there is little or no difference in the learning of P and U words.

These experiments suggest several reasons for the divergent conclusions of similar studies.

(1) Uncertainty of affective tone of experiences. Examples are those experiments in which the affective tone of the material is determined after the experiment only. Thus Fox had his *Ss* rate sonnets after they had attempted to learn them.[2]

(2) Considerable variation of the *S*'s acquaintance with the P and U stimuli. Examples are the experiments performed by Wohlgemuth[3] and Laird[4] in which recall of personal experiences was required. Thus event A might have been experienced recently 20 times and event B months ago. In other words the laws of frequency and recency should be considered as well as the law of intensity.

(3) Insufficient time interval before the measurement of recall. Chaney and

[2] C. Fox, The influence of the subjective preference on memory, *Brit. J. Psychol.*, 13, 1923, 398-404.

[3] A. Wohlgemuth, The influence of feeling on memory, *ibid.*, 405-429.

[4] D. Laird, The influence of likes and dislikes on memory as related to personality. *J. Exper. Psychol.*, 6, 1923, 294-300.

Lauer[5] found practically no difference in the recall of P and U words, using a 5-day interval. We, using a delay of 7, 14, 21, and 28 days, found a rather large difference. We do not believe that active forgetting operates only after a lapse of time, but that the recall method is too crude to be used as a measure of such factors except over relatively long periods. The amount of time required to recognize the words or the number of repetitions required to relearn the words would give a more delicate measure. Lynch,[6] using a recognition technique, found on a carefully evaluated list a tremendous drop in negative critical words after an interval of a week. He also reports very little difference in recognition between the 2- and the 3-weeks delay. This is exactly what we found to be true, using, not recognition, but recall.

(4) Different instructions to *Ss*. Thus in Experiment I in which instructions to master were used, there was little or no difference in the learning of P and U words. In the other two experiments in which instructions to master were not employed we found a difference.

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[5] R. M. Chaney, and A. R. Lauer, The influence of affective tone on learning and retention, *J. Educ. Psychol.*, 20, 1929, 287-291.

[6] C. A. Lynch, The memory values of certain alleged emotionally toned words, *J. Exper. Psychol.*, 15, 1932, 298-315.

THE RELATION OF TONAL VOLUME, INTENSITY, AND PITCH

By PAUL M. ZOLL, Harvard University

The purpose of this study is, first, to exhibit the instability of tonal volume as a category of sensory discrimination, as shown in the literature and by new experiments of the writer and, secondly, to make a preliminary report of some observations which show the dependence of sensory intensity upon frequency.

Tonal volume. Observers can make judgments of tonal volume, when the frequency of the tonal stimulus is varied, and also when its energy is varied. An *O* can remain fairly consistent in his judgments under the conditions of the psychophysical experiment during a long series of experimental sessions, giving quantitative results that fall into psychometric functions and even larger systematic relationships.[1] On the other hand, the concept of tonal volume has been criticized as lacking psychological reality or at least scientific definiteness.[2] Since the limens for volume are always greater than the limens for pitch when frequency is varied and greater than the limens of intensity when energy is varied, it can be argued that volume is simply a term for a high degree of subjective certainty about pitch or intensity, as the case may be, or some similarly indeterminate category of difference. The measure of the value of such a concept is its stability under varying experimental conditions. If tonal volume can be measured and quantitative functions for it can be made out, and if such results can be verified from laboratory to laboratory, then volume has attained a definite descriptive status, and it is defined by whatever formulation in one published account enables a second investigator to duplicate the experiment and verify the results. On the contrary, if consistent quantitative results are not easily obtained, or if, having been obtained, they are not capable of verification by a new *E* with new *O*s in another laboratory, then it is plain that the original observations depend upon some factor that is not yet explicit in the formulation of the experiment, and that the concept as yet lacks scientific reliability and value.

Table I shows values of the volumic limen as obtained by various investigators for a standard in the middle register. As long as the determinations by Rich[3] and Halverson[4] were not supplemented by others it could be said that there was a fair amount of consistency, that tonal volume was a fairly stable category. The results

* Accepted for publication October 15, 1932. This investigation was conducted under the direction of Professor E. G. Boring.

[1] *E.g.*, G. J. Rich (A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13-22) showed that a series of volumic limens, each derived from a psychometric function, may approximate a logarithmic function of frequency throughout a wide range of frequencies (110-6400 cycles).

[2] Cf. H. Banister, Auditory theory, a criticism of Professor Boring's hypothesis, this JOURNAL, 38, 1927, 438-439.

[3] G. J. Rich, *op. cit.;* A study of tonal attributes, *ibid.*, 30, 1919, 121-164.

[4] H. M. Halverson, Diotic tonal volumes as a function of difference of phase, *ibid.*, 33, 1922, 526-534; Tonal volume as a function of intensity, *ibid.*, 35, 1924, 361-367.

of Gundlach and Bentley,[8] however, disturb this harmony. They find very small values for the interval of uncertainty, and the data depend upon a body of experimentation much greater than Halverson's, which was merely preliminary to other experiments. Then some of the writer's own results now fall in with the range that

TABLE I

SUMMARY OF RESULTS OF VARIOUS INVESTIGATORS ON DIFFERENTIAL LIMENS FOR TONAL VOLUME

The last three columns show respectively the lower differential limen, the upper differential limen, and the interval of uncertainty

Investigator	Date	Instrument	Standard (cycles)	O	DL_L	DL_U	I.U.
Rich	1916	Variator	400	*B*	12.18	14.00	26.18
				D	12.66	14.88	27.54
Rich	1919	Variator with interference tubes.	550	*Da*			(26)*
				Di			(24)*
				G			(26)*
				W			(18)*
				Y			(20)*
				O	1.13	1.03	2.16
Halverson	1922	Variator	396	Av.			(28.6)*
Halverson	1924	Variator	396	Av.			(36.2)*
Gundlach and Bentley	1930	Variator	500	*B*	1.43	.62	2.05
				C	5.04	5.10	10.14
				F	.63	.13	.76
				G	.86	.78	1.64
				H	2.62	2.86	5.48
				I	3.02	3.45	6.47
				Mc	9.34	3.84	13.18
				M	.33	.57	.90
				S	2.44	3.41	5.85
				Z	1.47	.89	2.36
				Av.	2.72	2.17	4.88
Zoll (1)	1931	Variator	396	*F*	4.63	3.56	8.19
				V	4.02	2.37	6.39
				B	5.86	6.02	11.88
Zoll (2)	1932	Oscillator	345	*V*	9.32	6.75	16.07
Zoll (3)	1932	Oscillator	345	*El*, *Ob*	Undeterminable; excessive irregularities.		

* Where only one DL is available, the value for it has been doubled and entered in parentheses in the I.U. column, to allow a comparison of all the results in this column.

Gundlach and Bentley found. Altogether it can not be said that the table shows the kind of consistency wanted.

The complete data of the writer's experiments on the dependency of tonal volume upon frequency are given in Table II.

In a first experiment (Table II-1) a Stern variator was used in order that the

[8] R. Gundlach and M. Bentley, The dependence of tonal attributes upon phase, *ibid.*, 42, 1930, 519-543.

conditions of previous investigators might be duplicated. The standard tone was selected at 396 cycles. The method of constant stimuli was employed and the results were computed by Urban's tables. There were three trained *O*s: Mr. J. D. Frank (*F*), Dr. John Volkmann (*V*) and Professor E. G. Boring (*B*). The intervals of uncertainty proved to be much smaller than Rich's or Halverson's, although somewhat larger than those of Gundlach and Bentley. It is plain that the desired stability for tonal volume has not yet been achieved, although the results for these three *O*s are comparable and their volumic limens are clearly not to be confounded with limens for pitch, which the table shows for *F* and *V*.

TABLE II

VOLUMIC LIMENS AS A FUNCTION OF FREQUENCY

The last five columns show the measures of precision (h) for the lower and upper limens, the values of the lower and upper differential limens (DL), and the interval of uncertainty (I.U.). At the bottom of the tables are given the psychometric functions for *El* and *Ob* since their limens could not, on account of variability, be computed in Experiment 3. All values entered in the table (except no. of series) are for cycles per sec. In Experiment 1 two pitch limens were determined for the sake of comparison with the volumic limens.

Exp.	Standard (cycles)	Category	*O*	No. of series	h_L	h_U	DL_L	DL_U	I.U.
(1)	396	Volume	*F*	100	.10	.10	4.63	3.56	8.18
			V	100	.09	.08	4.02	2.38	6.39
			B	50	.08	.01	5.86	6.02	11.88
		Pitch	*F*	35	.35	.36	2.11	1.27	3.38
			V	50	.70	.55	1.03	0.56	1.59
(2)	345	Volume	*V*	50	.51	.50	9.32	6.75	16.07
(3)	345	Volume	6 untrained *O*s: no judgments obtainable						
			El	25	No limens obtainable because of excessive				
			Ob	25	irregularities.				

	El							*Ob*						
Comparison Stimuli	360	355	350	345	340	335	330	360	355	350	345	340	335	330
Percentages Larger	80	48	28	12	68	64	100	36	28	0	0	100	16	88
Percentages Smaller	20	8	12	12	4	8	0	60	28	68	8	0	72	8

In the second investigation (Table II-2) the source of sound was a single ear-phone placed at the level of the ears 3 ft. behind *O*'s head; the ear-phone was activated by a General Radio 508-A audio-oscillator placed in the adjoining room. The arrangement of the oscillator with the accessory apparatus, as shown in Fig. 1, permits the preparation of two tones, a standard and a comparison tone, differing in frequency or energy (the voltage across the ear-phone delivering the sound) or both, before the presentation of either tone. A standard tone with a frequency of 345 cycles and an output energy of 5.0 volts was chosen. *V* was the only *O*. It will be noticed that in this case the volumic limen is over twice as large as that determined for the same *O* in the first experiment. This increase indicates again the instability of tonal volume as a category of report.

In the third experiment (Table II-3) the apparatus was the same as in the second, except that the *O*'s head was kept immovable with the ear-phone only one foot be-

hind it, and that a standard tone was chosen of 345 cycles frequency and 2.0 volts output energy. In this work were used two trained *Os*—Dr. M. H. Elliott (*El*) and Mr. K. W. Oberlin (*Ob*)—and six untrained *Os*—Miss Duckering (*Du*), Miss Robinson (*Ro*), Miss Lathrop (*La*), Miss Biddle (*Bi*), Mr. Bolles (*Bo*), and Mr. Dearstyne (*De*). The attempt to determine volumic limens in this case was completely unsuccessful. The untrained *Os* could not understand what was meant by tonal volume; they identified volume with intensity or pitch or else were utterly unable to fix upon any criterion for judgment. The two trained *Os* were able, after

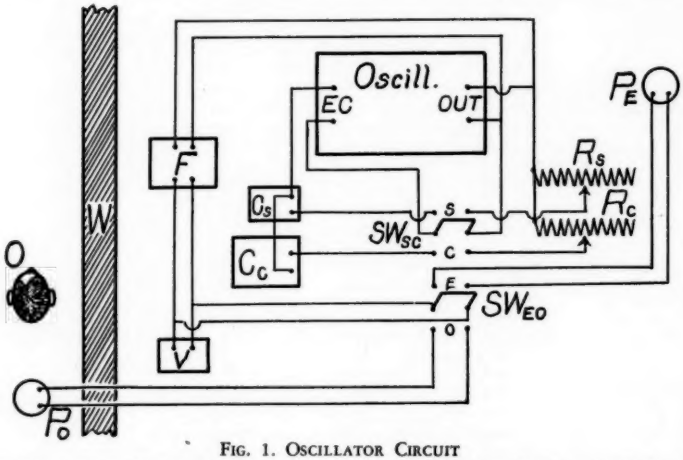

FIG. 1. OSCILLATOR CIRCUIT

Oscill.: audio-oscillator, 508-A.
Out: output leads from oscillator.
R_C: resistance for comparison stimulus.
R_S: resistance for standard stimulus.
EC: external capacitance leads from oscillator.
C_C: capacitance for comparison stimulus.
C_S: capacitance for standard stimulus.
SW_{SC}: double-throw switch, for standard stimulus (S) and for comparison stimulus (C).
F: low pass 500-cycle filter, to eliminate telephone diaphragm resonance at 800 cycles.
V: voltmeter across output to control intensity.
SW_{EO}: double-throw switch, for *E*'s phone (E) and for *O*'s phone (O)
P : Phone for *E*.
P : Phone for *O*.
O: observer.
W: wall of sound-proof room.

some preliminary practice, to recognize volume and give judgments upon it, although *El* had considerable difficulty with his reports and had little assurance that the basis of his judgments was 'volume.' The results obtained for *El* and *Ob* were still too irregular to allow limens to be computed from them; the percentages of report by them are given at the end of Table II to show the nature of the inversions encountered.

A consideration of the data of these three experiments—the variability, the irregularities, and even the indeterminateness of the volumic limen—indicates that some factor, implicit in and not completely controlled by the experimental procedure, has disturbed the consistency of the quantitative results.

Boring[6] has suggested that volume may depend upon the amplitude of the stimulus, intensity upon the energy of the stimulus, and pitch upon the frequency. Thus the apparent dependence of volume upon frequency might be due to the fact that for a constant expenditure of energy the amplitude would vary with the frequency. Such an hypothesis would explain the present situation where results are more consistent from *O* to *O* within the same experiment than from one experiment to another, since the expenditure of energy is constant but undetermined within a single experiment. But this hypothesis also requires a higher order of consistency when volume is investigated as a function of energy with frequency constant. Accordingly the writer undertook to repeat Halverson's results on the relation of tonal volume to intensity.[7]

In determining intensitive and volumic limens as a function of energy, a 6-volt audio-oscillator of a fixed 1000 cycle frequency was first used to activate an earphone placed behind the *O*. A slide rheostat inserted in the circuit permitted the presentation of weaker comparison tones only (as in Halverson's experiment); consequently, only lower limens could be procured. The changes in energy were read by a millimeter-scale on the rheostat from 0 mm. (the energy of the standard tone), to a minimal intensity of 200 mm. The resultant data are given in the first six lines of Table III.

The addition of another similar rheostat in a second part of this experiment made it possible to present more energetic as well as less energetic comparison stimuli. The limens, obtained from these results only in part by the use of Urban's tables, appear in the next four lines of Table III; those limens for which no *b*'s appear, were determined by Newhall's method of interpolation.[8]

In a second experiment the 508-A audio-oscillator with the apparatus described above for separate control of frequency and energy was used as the source of sound. With a standard tone of 345 cycles and 5.0 volts the volumic limen as a function of energy was determined for *V*. The results appear in the second section of Table III.

In a third experiment the 508-A audio-oscillator was used with a standard tone of 345 cycles and 2.0 volts. Since the six untrained *O*s could make no judgments at all upon volume, with variation either of frequency or energy, limens for intensity were procured instead. Satisfactory reports upon volume were, however, given by the trained *O*s, *El* and *Ob*. The results of this experiment appear in the third part of Table III.

It is not, of course, possible to compare the results, even of the first experiment, with Halverson's, since an arbitrary scale of mm., measuring a rheostat, gives no determinate statement of energy. Table III, however, shows at once that the desired consistency for these limens is not forthcoming. While the limens for volume are greater than the limens for intensity, there is great variability among the *O*s, and, what is even worse, the psychometric functions are so irregular and adventitiously variable, that in some cases it was not even practicable to compute the limens by Urban's method. We also see that untrained *O*s were again incapable of using volume as a consistent category of report.

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[6] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, *ibid.*, 27, 1926, 162-164.

[7] H. M. Halverson, *op. cit.*, 1924.

[8] S. M. Newhall, Linear interpolation *vs.* the constant process, this JOURNAL, 38, 1927, 390.

In short, all these experiments upon tonal volume, as a function of frequency and of energy, have exhibited the instability of tonal volume as a category of sensory discrimination.

Intensity. The irregularity of the results obtained for the discrimination of tonal volume as a function of frequency led to some observations upon the stability of sensory intensity with the variation of frequency. It is known that sensory intensity changes when the pressure of the stimulus (dynes/cm.^{2}) is kept constant and the frequency is changed.[9] In the frequency range of the present investigation the rule is: the greater the frequency the louder the tone if pressure is constant—a rule that resembles Helmholtz' "intrinsic intensity" of high pitches.

TABLE III

VOLUMIC AND INTENSIVE LIMENS AS A FUNCTION OF ENERGY

See Table II for the meaning of the symbols. In Experiment 1 energy was varied with a rheostat and values entered in the table are mm. of the rheostat scale. In Experiments 2 and 3 energy was measured by the output voltage of the oscillator and the values are in volts.

Exp.	Standard	Category	No. of series	O	h_L	h_U	DL_L	DL_U	I.U.
(1)	0 mm.	Volume	50	*F*	.01		162.64		
			50	*V*	.02		47.21		
			50	*B*	.02		27.76		
		Intensity	25	*F*	.01		43.02		
			25	*V*	.09		2.15		
			25	*B*	.03		9.81		
	0 mm.	Volume	30	*F*			157.00	200.00	
			25	*V*			68.00	78.00	
		Intensity	25	*F*			114.00	97.00	
			25	*V*	.03	.03	14.60	15.70	
(2)	5 v.	Volume	50	*V*	2.40	2.40	.36	.40	0.77
(3)	2 v.	Intensity	25	*Du*	4.23	4.63	.10	.11	0.21
			25	*Ro*	2.33	2.17	.12	.02	0.14
			25	*La*	3.79	3.39	−.01	.09	0.09
			25	*Bi*	2.27	2.83	.04	.11	0.14
			25	*Bo*	3.30	3.99	.15	.11	0.26
			25	*De*	3.51	3.60	.04	.15	0.19
		Volume	25	*El*	1.35	1.44	.57	.60	1.18
			25	*Ob*	3.39	3.72	.26	.22	0.48

The 508-A audio-oscillator with separate control of frequency and energy was used as in Fig. 1. A tone of 345 cycles and 2.0 volts was kept throughout as the standard. The *Os* were those of the third experiment upon volume.

In a first experiment reports on intensive differences were procured for *Bo, De, La,* and *El,* with comparison tones varying in frequency and of a fixed energy of 2.0 volts for both standard and comparison. If variation of frequency alone had no effect upon intensity, we should expect a large percentage of 'equal' reports for all stimuli with only a small number of equally distributed judgments of 'louder'

[9] B. A. Kingsbury, A direct comparison of the loudness of pure tones, *Phys. Rev.,* 2 ser., 29, 1927, 588-600; D. MacKenzie, The relative sensitivity of the ear at different levels of loudness, *ibid.,* 2 ser., 20, 1922, 331-348.

TABLE IV

PERCENTAGES OF REPORT OF 'LOUDER,' 'EQUAL,' AND 'SOFTER' AS A FUNCTION OF FREQUENCY
The standard stimulus is 345 cycles at 2.0 volts; the comparison stimuli vary from 330 to 360 cycles at 2.0 volts.

O	Report	Comparison stimuli						
		360	355	350	345	340	335	330
Bo	Louder	84	88	0	0	56	0	76
	Equal	16	12	48	96	40	40	24
	Softer	0	0	52	4	4	60	0
De	Louder	84	52	12	8	44	4	84
	Equal	16	8	20	72	20	8	8
	Softer	0	40	68	20	36	88	8
El	Louder	36	52	16	0	32	0	32
	Equal	48	48	76	96	56	36	68
	Softer	16	0	8	4	12	64	0
La	Louder	95	60	95	10	40	5	0
	Equal	0	0	0	70	0	0	5
	Softer	5	40	5	20	60	95	95

and 'softer.' Table IV, however, in which these percentages are recorded, shows that this does not occur. Of the four *O*s, *El* alone reported a large amount of 'equal' judgments, and, even in his case, the percentages of reports of 'louder' and 'softer' for several frequencies are significantly different. All the *O*s agreed in reporting tones of 360 and 355 cycles as 'louder' most of the time, 345 as 'equal,' and 335 as 'softer,' in comparison with the standard tone of 345 cycles and of equal voltage. This result clearly indicates that tones of 360, 355, and 335 cycles differ in sensory intensity from one of 345, even though the output energy across the phone is maintained constant.

TABLE V

INTENSITY AS A FUNCTION OF VOLTAGE WITH FREQUENCY CONSTANT

O	Fixed comparison (cycles)	Report	Comparison stimuli (volts)					h	DL	I.U.	S.E.
			1.8	1.9	2.0	2.1	2.2				
Du	330	Louder	0	4	32	32	64	3.89	.14	.17	2.06
		Equal	20	16	44	44	20				
		Softer	80	80	24	24	16	3.72	.03		
	360	Louder	16	44	52	68	80	2.96	−.01	.22	1.88
		Equal	28	40	32	24	20				
		Softer	56	16	16	8	0	3.58	.21		
	335	Louder	4	12	24	24	40	2.08	.30	.27	2.13
		Equal	12	28	20	32	24				
		Softer	84	60	56	44	36	3.93	−.04		
Ro	355	Louder	32	20	72	68	84	2.92	−.04	.41	1.81
		Equal	44	56	16	20	12				
		Softer	24	24	12	12	4	1.65	.45		

To determine this point more conclusively intensitive limens for *Du* and *Ro* were determined with the comparison stimuli at different frequencies from the standard, and the effect of difference in frequency was noted (see Table V). For *Du* the comparison stimuli were fixed at frequencies 330 and 360 cycles, and for *Ro* at 335 and 355 cycles, with the standard always, of course, at 345 cycles and 2.0 volts. It is seen that tones of 360 and 355 cycles are much louder and tones of 330 and 335 are much softer than the standard of 345. These results, except perhaps those with 330 cycles, are in agreement with the conclusions drawn from the results given in Table IV.

Thus it appears that these results are in accord with the general rule that higher frequencies, within the lower frequency ranges, tend to be more intense.

THE EFFECT OF HUNGER ON VARIABILITY OF PERFORMANCE

By M. H. ELLIOTT, Harvard University

The usual learning experiment with rats may be considered to deal with the changes in the initial random behavior which result from differential rewards; that is to say, certain of the responses are rewarded and others are punished or not rewarded. The distinguishing feature of the present experiment lies in the fact that the animals are always rewarded in the same manner regardless of the path taken. Under these conditions the question arises as to whether variations in the strength of hunger will be followed by changes in the variability of performance.

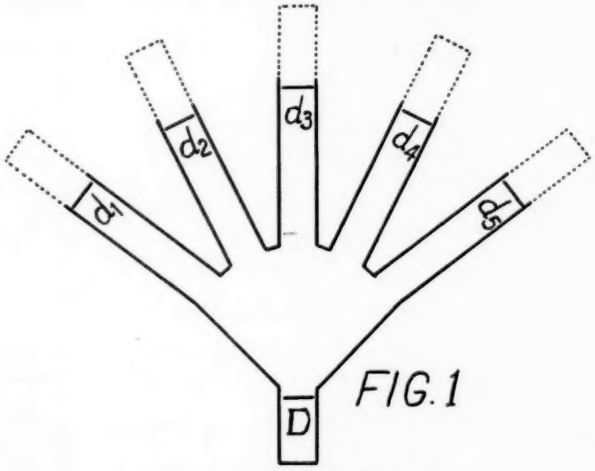

FIG. 1. FLOOR-PLAN OF APPARATUS

Apparatus. The floor-plan of the apparatus is shown in Fig. 1. There were 5 alleys leading from the large end of a wedge-shaped compartment. The rat was placed in the entrance-box. When the door (D) was opened the rat could enter the choice-compartment and from there proceed down any one of the 5 alleys. At the end of each alley was a small wire cage containing food. When the rat had entered one of these food-cages the appropriate door (d_1, d_2, etc.) was closed.

The sides of the apparatus were made of sheet metal and the top was covered with wire-mesh. The flooring was made of rubber composition. The doors were sheets

* Accepted for publication October 31, 1932.

of metal sliding in felt-lined vertical grooves. These doors were operated by strings passing over pulleys and were practically noiseless. Indirect lighting provided a dim and fairly uniform illumination. The apparatus was placed so that the entrance-box protruded through an opening in a large wall-board screen. *E* was stationed behind this screen and watched the animal through a narrow slit.

Special precautions were taken to prevent a rat from trailing the path of the previous rat. The flooring was thoroughly brushed with a damp brush after each trial and clean flooring was laid down after every four or five trials.

Daily procedure. Each rat was given two trials per day and was allowed to eat for 2½ min. at the end of each trial. A period of 15–20 min. intervened between the two trials and other rats were run during this time. After the two trials the rat was returned to the living-cage in another room and 45 min. later was given a measured amount of additional food to make up the daily ration.

Variations in hunger. The variations in hunger were determined by the amount of food given at the later feeding in the living-cages. Four different degrees of hunger were used. Three of these corresponded to daily rations of 4, 6, and 10 grm.

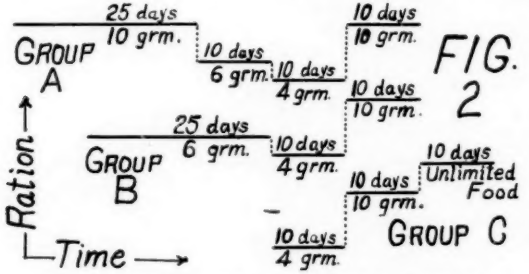

FIG. 2. RATIONING PLAN FOR GROUPS A, B, AND C

per animal. The fourth and least degree of hunger was produced by allowing the animal to eat freely from 45 min. after the trials on one day until 45 min. before the trials on the next day.

Experimental groups. The rats used in the experiment varied in age from 85 to 100 days at the beginning of training.

Fig. 2 shows the way in which the rations were varied in successive increments of time for the three groups, A (15 rats), B (14 rats), and C (20 rats). Each day represents two trials. It will be seen that Groups A and B were given similar treatment in that hunger was increased in successive periods until after 10 days on the 4-grm. ration and then decreased. Moreover, conditions for these two groups were identical for the last 30 days of training. Group C, on the other hand, started with maximal hunger, and then the hunger was decreased to a minimum in the last 10 days.

Statistical treatment. The data obtained consisted of records of the alleys chosen by each rat in the various trials. The alleys were arbitrarily numbered from 1 to 5. Thus Rat No. 1 of Group A gave the following distribution of choices during the initial period of training on 10 grm. per day:

Alley No.	1	2	3	4	5
Choices	2	2	11	14	21

Treating this array as an ordinary distribution one obtains a standard deviation of 1.088, which may be considered as one measure of the variability of this rat's performance during this period. In this way a variability score (σ) was obtained for each rat for each of the experimental conditions. These derived measures were then treated in the usual manner: means and standard deviations were computed and comparisons made, as shown in the tables. The appropriate formulae for use with a small number of cases were used. In obtaining the σ diff. the complete formula for correlated measures was used.

In order to approach the data from a slightly different point of view a statistical study was made of the relative preference for paths. This result was achieved by taking for each rat the path most often selected during the whole training period and then determining the percentages of choices of this path under the various experimental conditions. Thus it was found that over the whole period of training Rat No. 1 of Group A entered Alley 4 more often than any other. From the distribution given above we find that in the initial period of training on 10 grm. per day this rat selected Alley 4 28% of the time. In the following periods the percentages

TABLE I

VARIABILITY OF PERFORMANCE

Group		10 grm.	6 grm.	4 grm.	Terminal 10 grm.
A	Mean	1.459	1.283	.998	.848
	S.D.	.448	.560	.620	.623
B	Mean		1.452	1.081	1.189
	S.D.		.333	.735	.560

were respectively, 25, 70, and 80. From these percentages of choices of preferred path for each rat distributions were made for the different groups under the various experimental conditions. Means and standard deviations were computed and comparisons made as before.

Although this second procedure does not give so direct or dependable a measure of variability of performance, it nevertheless provides an indication of the consistency of preference.

Results. Table I presents the means and standard deviations of the variability of performance for Groups A and B under the various conditions of hunger. It should be remembered that these means are means of distributions of standard deviations. Likewise, the standard deviations are standard deviations of distributions of standard deviations.

It will be seen that the variability (mean) becomes smaller with the increase in hunger. Contrary to expectations, however, the variability did not return to its former level when the hunger was decreased (terminal 10 grm.)

Table II presents for the various conditions of Groups A and B the differences in the mean variability of performance and also gives measures of the significance of these differences.

The data in Table II show that there is a fairly significant decrease in the

variability of performance when the hunger is made sufficiently strong. This change in variability appears to have a certain permanence as it is not greatly affected by the later decrease in hunger.

Group C was intended to serve as a control to determine whether the change in variability could be ascribed to mere repetition. It will be remembered that this group

TABLE II

DIFFERENCE IN VARIABILITY OF PERFORMANCE

Group		10 vs. 6 grm.	10 vs. 4 grm.	10 vs. term. 10 grm.	4 vs. term. 10 grm.
A	Diff.	.176	.461	.611	.150
	D/σ diff.	1.354	3.013	3.748	1.250
		6 vs. 4 grm.	6 vs. term. 10 grm.	4 vs. term. 10 grm.	
B	Diff.	.371	.263	.108	
	D/σ diff.	2.876	2.121	.651	

started with maximal hunger and that the hunger was *decreased* in successive periods. The results for Group C are shown in Tables III and IV.

It is apparent that the differences in Table IV can not safely be regarded as significant. There is little evidence of an effect on variability resulting from repetition. As a further check the variability of the combined Groups A and B on the 4-grm. ration was compared with that of Group C on the 4-grm. ration. This comparison

TABLE III

VARIABILITY OF PERFORMANCE

	4 gms.	10 gms.	'Unlimited' food
Mean	.903	.788	1.092
S.D.	.513	.721	.708

gave a difference of 0.141 with the D/σ diff. of 0.671. Again there is indication that the amount of previous training had little effect on the variability of performance.

The data on the percentages of choices of path most preferred over the whole training period are summarized in Table V.

The differences given in Table VI appear to have approximately the same significance as those in Tables II and IV. The decrease in variability of performance

TABLE IV

DIFFERENCE IN VARIABILITY OF PERFORMANCE

	4 vs. 10 grm.	4 grm. vs. unlimited	10 grm. vs. unlimited
Diff.	.115	.189	.304
D/σ diff.	.248	.390	.704

shown in the earlier tables is paralleled by a tendency for each rat to select more often some particular path. This relative preference is little affected by differences in training prior to the period of maximal hunger. After the period of maximal hunger a decrease in hunger does not change the preference.

Conclusions. When rats in the multiple-choice apparatus were sufficiently hungry, each rat tended to select some particular path on successive occasions in spite of the fact that all choices were equally rewarded. In the case of Group A it was found that many repetitions with insufficient hunger had little effect on the variability of performance. The variability of performance is inversely related to hunger but the change in variability of performance is not reversible. That is to say, after variability has been reduced under conditions of strong hunger, it is found that decreases of hunger have little effect. Furthermore, the amount of previous training does not greatly affect the variability under any particular degree of hunger.

From these data some of the more important comparisons are given in Table VI.

TABLE V

PERCENTAGE OF CHOICE OF PREFERRED PATH

Group		10 grm.	6 grm.	4 grm.	10 grm.	Unlimited
A	M	51.3	54.7	66.8	70.0	
	S.D.	20.1	22.6	25.3	25.8	
B	M		50.9	70.0	68.6	
	S.D.		21.1	26.9	20.3	
A+B	M		52.8	68.6	69.2	
	S.D.		21.6	25.7	22.4	
C	M			74.3	81.0	71.5
	S.D.			26.5	20.1	28.8

· *Discussion.* The relationships between hunger and variability of performance do not appear to have been explicitly treated in the literature. Experiments with activity-wheels indicate that there is a relationship between hunger and *amount* of activity. Moreover, various studies have shown that efficiency of learning in the maze is positively related to strength of hunger; but this relationship also must be due to the greater activity of the hungrier animals. Neither type of study then has been concerned with stability of performance. Stated roughly it can be said that the experiments with activity-wheels and mazes have been dealing with the number of actions per minute as related to hunger. On the other hand, the present experiment has dealt with the persistence of some particular sequence of actions as it is related to hunger.

TABLE VI

DIFFERENCES IN PERCENTAGE OF CHOICE

Comparison	Diff.	D/σ diff.
Group A+B, 6 vs. 4 grm.	15.8	2.424
Group A+B, 4 vs. terminal 10 grm.	0.6	.091
Group A, initial 10 vs. A+B, terminal 10 grm.	17.9	2.609
Group C, 4 vs. 10 grm.	6.7	.903
Group C, 4 grm. vs. unlimited food	2.8	.321
Group A+B, 4 grm. vs. Group C, 4 grm.	5.7	.730
Group C, 10 grm. vs. Group A, initial 10 grm.	29.7	4.329

The usual trial-and-error account of learning seems to imply that the state of

motivation results in random and variable behavior. Presumably, there is also the implication that the stronger the state of motivation, the more variable will be the behavior. However reasonable such a relationship may appear, if it exists at all, it can exist only until the situation yields some reward, for the results of the present experiment indicate that after the reward there is an inverse relationship between strength of hunger and variability of performance. Moreover, it seems that this relationship is not reversible.

This irreversibility of the relationship, shown by the fact that, after the behavior had been affected by strong hunger, it was not changed by later decreases of hunger, brings the experiment into line with other learning experiments. Gilhousen[1] and Krechevsky and Honzik[2] have shown that repetition has the effect of fixating habits. The results of this experiment indicate the possibility that strong motivation is another fixating agent.

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[1] H. C. Gilhousen, An investigation of insight in rats, *Science*, 73, 1931, 711-712.
[2] I. Krechevsky and C. H. Honzik, Fixation in the rat, *Univ. Calif. Publ. Psychol.*, 6, 1932, 13-26.

THE RELATIONSHIP BETWEEN TRANSFER OF LEARNING AND AGE OF PREVIOUS ASSOCIATIONS

By STEUART H. BRITT, Washington University

The phenomenon of transfer of training, as treated by Carr,[1] refers to the utilization of past experiences in dealing with new situations. The effect of training in the earlier activity may be (1) positive, facilitating the formation of the subsequent habit; (2) negative, inhibiting the formation of the second habit; or (3) of no effect at all.

There may be several factors in the first learning situation which will be conducive to positive transfer and several other factors conducive to negative transfer. Whether the learning of the first problem aids, hinders, or has no influence upon the learning of the second problem is probably a matter of the balance between the positive and negative transfer elements.

The present investigation represents a study of the relationship between the transfer effect of one maze upon another and the age of the associations in the first problem. In another laboratory experiment, concerned with Jost's Law,[2] it was discovered that two groups of *Ss* differed greatly in the efficiency exhibited in mastering a second maze when, in one case, the first problem had been learned to a definite criterion immediately preceding the second, and, in the other case, the immediately preceding activity had been the relearning of the first maze to the same criterion after an interval of 48 days. A difference between the two age-groups in the ease with which the second maze was learned is what one would have expected. However, as a control, it was thought worth while to have a third group master the second maze problem without having had any previous maze experience. This was done in order to determine the exact amount of difference in transfer when the same level of efficiency has been reached on the first maze problem but when the associations in one group are older than in another group.

Conditions of experiment. The learning problems were two stylus mazes, I and II. Maze I was the original learning problem, a brass maze, approximately 14½ by 14¼ in., of the same pattern as that used by Carr in his experiment on visual guidance.[3] Maze II, the second problem, was made of fiber-board, approximately 12 by 12 in. The pattern of each maze is shown in Fig. 1.

The *Ss* were 90 students of Washington University, none of whom had had previous maze experience. They were divided in random order into 3 groups of 30 each. Men and women were approximately equally represented in each group.

One group of *Ss* learned the original problem, after a time interval of 48 days relearned the same problem, and immediately mastered the second problem. The second group learned the original problem, and immediately learned the second maze. These two experimental groups reached the same level of performance, *i.e.* 3 suc-

* Accepted for publication August 1, 1932.

[1] H. A. Carr, *Psychology,* 1925, 99, 237 ff.

[2] S. H. Britt and M. E. Bunch, Jost's Law and retroactive inhibition, this JOURNAL, 46, 1934, (April).

[3] H. A. Carr, The influence of visual guidance in maze learning, *J. Exper. Psychol.,* 4, 1921, 399-417.

cessive trials without error, in the first problem immediately before learning the second problem; but they differed in the fact that Group 1 had learned the first maze 48 days before Group 2. Group 3, as a control, learned only the second maze. This control served as a standard with which to compare the learning scores on the second problem of the two transfer groups.

Three measures of learning efficiency were employed for each maze problem: the total number of trials, the total length of time in seconds (both of these records including the last three perfect trials), and the total number of errors. The particular kinds of errors were also recorded, designated as A error, B error, and C error. An "A" error consisted of retracing the stylus anywhere in the true pathway; a "B" error consisted of entering a blind alley or retracing in a blind alley after

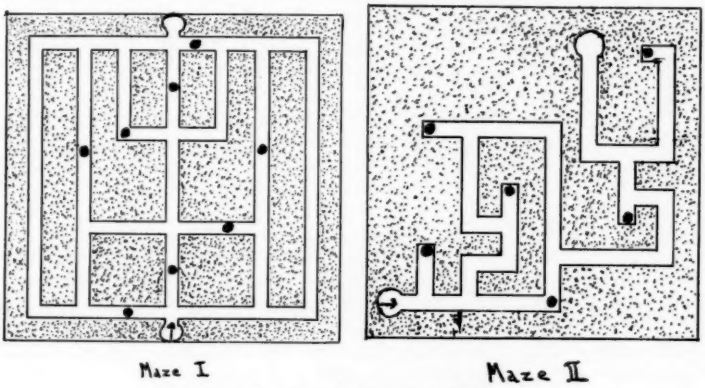

FIG. 1. SHOWING PATTERNS OF MAZES
Maze I, original problem, made of brass: Maze II, secondary problem, made of fiber-board.

having started out; and a "C" error consisted of touching the end, a stop, in a blind alley.

Results. The experimental results are presented under three separate headings: (1) the amount of transfer in the older age-group, Group 1; (2) the amount of transfer in the younger age-group, Group 2; and (3) the relative amounts of transfer in the older and younger age-groups, Groups 1 and 2.

(1) The amount of transfer in the older age-group, Group 1. The first results to be considered are those where Maze II was introduced after the original problem, Maze I, had been brought up to the level of complete mastery 48 days after original learning. This means that the older age-group, Group 1, is to be compared with the control group, Group 3.

According to every type of measurement, Group 1 was far superior to Group 3, and this shows a great amount of positive transfer for Group 1 to the second maze problem. Group 1 averaged 632.83 sec. to learn Maze II, whereas Group 3 averaged 1,240.47 sec., which means that Group 1 required less time, on the average, than Group 3 by 607.64 sec. Similarly, Group 1 required, on the average, only 21.63 trials, while Group 3 took 37.03 trials, a difference in favor of Group 1 of

15.40 trials. As to total errors, the average score for Group 1 was 203.63, but for Group 3 was 533.90, an average-difference of 330.27 total errors. There was the same relationship between the two groups for the three types of errors: Group 1 was superior to Group 3, in averages, by 104.64 A errors, by 128.33 B errors, and by 97.30 C errors. With every type of measurement, the ratio of each difference to the sigma of that difference indicated that the chances were 99.9 in 100 that the difference between the two groups was a true one.

(2) The amount of transfer in the younger age-group, Group 2. The second set of results are those obtained under the conditions in which Maze II was given immediately after the complete mastery of the original problem, Maze I. Hence, the younger age-group, Group 2, is to be compared with the control group, Group 3.

In general, Group 2 was slightly superior to Group 3, indicating some positive transfer for Group 2 to the second problem. The differences between the two groups as to time scores were slight: an average of 1,159.10 sec. for Group 2, and of 1,240.47 sec. for Group 3, which means that Group 2 required less time, on the average, than Group 3 by 81.37 sec. On the basis of trials, Group 2 was inferior: 39.20 trials, as compared with 37.03 for Group 3, a difference in favor of the control group of 2.17 trials. However, with regard to errors, Group 2 was better than Group 3 by every type of measurement. The average total-error score for Group 2 was 478.46, and 533.90 for Group 3, an average-difference of 55.44 total errors. Group 2 also made a better average score than Group 3 by 10.14 A errors, by 29.83 B errors, and by 16.53 C errors. The following figures show the chances in 100 that, if the experiment were repeated, the direction of the difference would be confirmed: over 65 for time; over 62 for trials; over 73 for total errors; 62 for A errors; over 82 for B errors; over 74 for C errors.

(3) The relative amounts of transfer in the older and younger age-groups, Groups 1 and 2. It has been shown that there was transfer to Maze II in the case of both the older age-group, Group 1, and the younger age-group, Group 2. The relative amounts of transfer in the older and younger age-groups are obtained by a comparison of the differences between Groups 1 and 3 (showing transfer for the older age-group) with those between Groups 2 and 3 (showing transfer for the younger age-group). These data have been presented in the preceding sections. The results of the comparison are given graphically in Fig. 2. The differences between each of the transfer groups and the control group are presented as a percentage decrease or increase with respect to the learning scores of the control group. For each pair of blocks in Fig. 2, the first in each case (lettered O) represents the percentage of transfer in the older age-group, *i.e.* the percentage of transfer of Group 1 as compared with the control, Group 3; while the second block in each case (lettered Y) represents the percentage of transfer in the younger age-group, *i.e.* the percentage of transfer of Group 2 as compared with the control, Group 3.

According to every type of measurement employed, there was a far greater amount of positive transfer (superiority to the control group) for the older age-group than for the younger. The percentage of transfer for Group 1 was 49.0% in time scores, but the percentage of transfer for Group 2 in time scores was only 6.6%. On the basis of trials, the percentage of transfer for Group 1 was 41.6%; on the other hand, the control group was somewhat superior to Group 2 in trials, by 5.9%. The other percentages of transfer were as follows: in total errors, 61.9% for Group 1, 10.4% for Group 2—in A errors, 55.1% for Group 1, 5.3% for Group 2—in B errors, 64.9% for Group 1, 15.1% for Group 2—in C errors, 66.6% for Group 1, 11.3% for Group 2.

A second method of determining the relative amounts of transfer in the two age-groups is to compare the raw learning scores of Groups 1 and 2. On this basis, Group 1 was superior to Group 2 in learning Maze II by the following percentages: 83.2% in time; 81.2% in trials; 135.0% in total errors; and 110.6%, 141.7%, and 168.0% in A, B, and C errors respectively. In each case the chances were 99.9 in 100 that there was a true difference between the two groups.

Conclusions. This experiment concerns the relationship between the age of the associations for a learning problem and the amount of positive transfer to a second

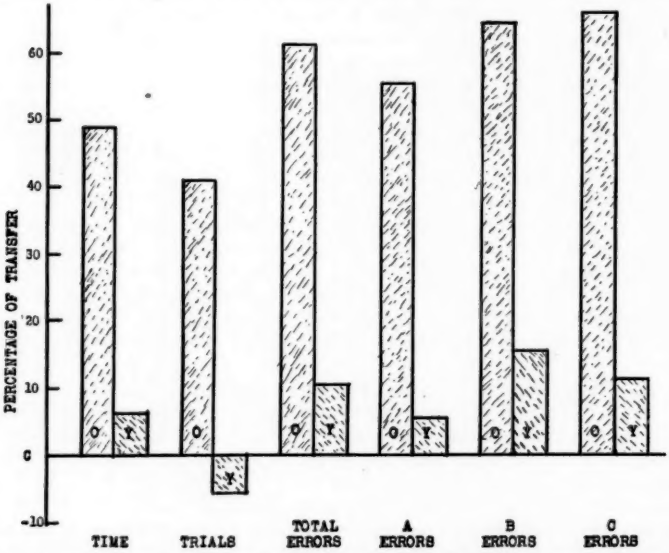

FIG. 2. PERCENTAGE OF TRANSFER FOR GROUP 1 AS COMPARED WITH GROUP 2 The base line marked C represents the learning scores of the control group, Group 3. These scores are used as a standard with which to compare the percentage of transfer of the experimental groups, Groups 1 and 2. Blocks lettered "O" (older) indicate the percentage of transfer of Group 1 as compared with Group 3. Blocks lettered "Y" (younger) indicate the percentage of transfer of Group 2 as compared with Group 3.

learning problem. The results show that if the associations for a learning problem are of the same strength but are of unequal age, the older association is more subject to positive transfer to a second problem than is the younger association.

From a theoretical standpoint, this conclusion does not imply that the amount of positive transfer is caused by the age of the association. It is simply a statement of the relationship between age differences and transfer. The greater amount of positive transfer for the older association may be due to a large number of factors which necessarily vary with the age of the association, such factors as distribution of effort, additional practice, greater familiarity, and confidence.

EQUIVALENCE OF BRIGHTNESSES ACROSS MODALITIES

By NATHAN E. COHEN, Harvard University

Von Hornbostel,[1] in a recent article on olfactory brightness, raised the question as to whether the brightnesses of the various modalities are identical or merely analogous. In support of the alternative of identity he described an experiment involving, first, the determination by direct comparison of a tone and a gray, each equal in brightness to a given odor, and then, the determination of a gray equal in brightness to the tone. He found the grays determined by these two procedures to be of practically the same stimulus value.[2] These results clearly suggest that, at least in the case of tones, odors, and grays, brightness is a common attribute in the following sense. Supposing b to represent a point on the visual scale of brightness and b' the point on the auditory or olfactory scale, a point which is called identical in comparison, then the replacement of b by b' should cause no change in the continuity of the visual brightness scale and vice versa.

The importance of Von Hornbostel's results systematically made it desirable to verify them to make sure that they were not due to the operation of some experimental artifact. The present investigation was carried out with stimuli similar to those used by Von Hornbostel but the results obtained were quite different. It will help clarity to distinguish Parts I, II, and III.

Part I. Part I of the investigation consisted in obtaining a series of comparative judgments between an odor and a range of tones. The constant olfactory stimulus was 5 cc. of benzol, and a Stern variator with a range of 150-300 cycles was used as the variable. *O* was seated 10 ft. from the variator with his back to it. The bottle containing the odor was placed on a stand in front of him. The following instructions were given him: "At the signal 'ready' raise the stopper from the bottle and sniff the odor. After a brief interval a tone will be sounded. Immediately judge to yourself as to whether the tone is greater, equal, or less in brightness than the odor. State your judgment to the *E*." *E* started at the upper end of

* Accepted for publication June 17, 1932.

[1] E. M. von Hornbostel, Über Geruchshelligkeit, *Arch. f. d. ges. Physiol.*, 227, 1931, 517-538.

[2] "Vergleicht man etwa einen bestimmten Ton mittlerer Lage, z.B. 400 Hertz, mit Schwarz-Weiss-Mischungen auf dem Farbenkreisel, so erscheint ein schwärzliches Grau viel zu dunkel, ein weissliches Grau viel zu hell, und indem man sich von beiden Seiten her annähert, gelangt man schliesslich zu einem mittleren Grau, das zu dem Ton am besten zu passen scheint. Ebenso kann zu einem Geruch das gleichhelle Grau oder der gleichhelle Ton eingestellt werden. Sucht man dann zu dem so gefundenen Ton das zu ihm passende Grau, so erhält man sehr annähernd denselben Kreiselwert wie direkt vom Geruch aus, z.B.: Benzol = 40° Weiss; Benzol = 220 Hertz; 220 Hertz = 41° Weiss. Durch solche Äquivalenzbestimmungen, im Dreieck erscheint die Annahme begrundet, dass Helligkeit nicht nur eine analoge, sondern eine identische Seite der Phänomene der verschiedenen Sinnesgebiete ist. Dann wird aber auch in den zugrunde liegenden Nervenvorgängen ein von der Sinnesmodalität unabhangiger Parameter für die Helligkeit vorauszusetzen sein." *Op. cit.*, 519.

the tonal range and worked down to find, first, a higher, and then lower, level of approximate equivalence to the brightness of the constant odor. This range of tones thus obtained was divided into equal steps and the tones were then presented in a haphazard order in the method of constant stimuli. Urban's tables were used throughout the experiment in determining the point of subjective equality (P.S.E.).

The results obtained from the two *O*s, *WA* and *SP*, used throughout the experiment, were as follows:

WA in a total series of 125 judgments gave a P.S.E. of 159 cycles.
SP in a total series of 200 judgments gave a P.S.E. of 267 cycles.

Part II. Part II, carried on a week later, consisted in obtaining a series of comparative judgments between an odor and a range of grays. The constant was again 5 cc. of benzol, and the grays produced by a color-wheel were used as the variable. *O* was seated 5 ft. from the color-wheel and a screen was used to prevent him from noting the changes made. Lighting was artificial and constant. The bottle containing the odor was placed on a stand alongside *O*. The following instructions were given: "At the signal 'ready' raise the stopper of the bottle and sniff the odor. After a brief interval a disk will be presented on the color-wheel. Immediately judge to yourself as to whether the disk is greater, equal, or less in brightness than the odor. State your judgment to *E*." *SP* was not started at the top of the range, but at 90° white and worked down from this point. *WA* was started at 240° white and worked down until the higher and lower levels were obtained. Upon the obtaining of the range the treatment was the same as in Part I.

The results obtained from the two *O*s were as follows:

WA in a total series of 125 judgments gave a P.S.E. of 83° white.
SP in a total series of 150 judgments gave a P.S.E. of 85° white.

Part III. Part III, carried on a week later, consisted in obtaining a series of comparative judgments between a gray and a range of tones. The P.S.E. obtained for each *O* in Part II was used as the constant (*WA*, 83° white; *SP*, 85° white) and for preliminary exploration tones of a Stern variator with a range of 500-1000 cycles were used as the variable. The conditions were similar to those in Parts I and II. The following instructions were given: "At the signal 'ready' a disk will be presented on the color-wheel. After a brief interval a tone will be sounded. Immediately judge to yourself as to whether the tone is greater, equal, or less in brightness than the disk. State your judgment to *E*." *SP* started at 900 cycles and worked down until a higher and lower level of approximate equivalence were obtained. *WA* started at 920 cycles and worked down, but a lower level could not be found on the range of the variator used. Another variator with a range of 200-400 cycles was substituted and the same procedure followed.

The results obtained were as follows:

WA in a total series of 125 judgments gave a P.S.E. of 300 cycles.
SP in a total series of 150 judgments gave a P.S.E. of 804 cycles.

The results of the three determinations of equivalents may be summarized as follows:

Pitch	(1)	Odor	(2)	Gray	(3)	Pitch
159 cycles	←	benzol	→	83° white	→	300 cycles
267 cycles	←	benzol	→	85° white	→	804 cycles

The arrows point from the standard of brightness toward the variable that was subjectively equated to the standard. The numbers in parentheses show the order of the experiments. Benzol was first equated to a pitch and then to a gray, which was in turn equated to a pitch. Since the arrows indicate equivalences, the frequencies at the two ends of each line should be the same. There is, however, about an octave of difference in the first case, and an octave-plus-a-fifth of difference in the second case.

These results indicate too great a discrepancy to render the hypothesis of identity plausible. The question therefore remains as to how the *Os* make their comparative judgments. A clue is provided by the recent work upon 'absolute' judgments[3] when considered with the indubitable fact of transposition.[4] Previous stimulation may have been sufficient for the establishment of the attitude necessary for 'absolute' judgments for each of the three continua of brightness. It would not be unreasonable then to suppose that cross-modality comparison should be based (physiologically, if not introspectively) upon relative positions within different 'absolute' scales. According to this view equation with respect to brightness of two experiences of different modalities would involve nothing more than the identity of relative positions upon two wholly independent scales.

This hypothesis is adequate not only to explain the general results of the experiment, but also the difference in results in the case of the two *Os*. Clearly the absolute series will depend in varying degree upon past experience and present range of stimuli.[5] Thus we may expect two different types of judgment, as past experience preponderates over present range of stimulation or the converse. If we assume that for *WA* the importance of past experience is greater than for *SP*, we can understand that for the former the results will be fairly constant, whereas, for the latter a shift to most any position along the scale may be obtained.

This hypothesis is further confirmed by the following observations: Three *Os* were given a tone of 220 cycles and asked whether it was high, low, or medium in brightness. Two of the *Os* reported it as medium, the third as lying between low and medium. The same type of observation was then made on the scale of grays. Eight *Os* were asked to judge whether 85° white is 'high,' 'low,' or 'medium' in brightness. Six reported it as medium, one as high, and the other as low. Thus we see that, if a medium tone or gray is given alone, it generally tends to be reported as medium, that is to say, the absolute judgment tends to be determined by past experience. Another set of observations consisted of presenting to a group of *Os* the upper 18 papers of a series of 50 grays. The *Os* were asked to select the gray that was equal in brightness to a given tone of 220 cycles. The papers selected ranged between the second and the fifteenth. Three weeks later the same *Os* were asked to perform a similar task: the tone was again 220 cycles but the grays were the lower 18 of the 50 papers. Nevertheless, all the *Os* made a selection, and the choices ranged between the thirty-eighth and forty-eighth papers. Thus when a gray is presented as part of a series, its 'absolute' brightness for all *Os* appears to be mainly a function of the series.

[3] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493.
[4] W. Köhler, *Gestalt Psychology*, 1929, 214.
[5] Wever and Zener, *op. cit.*

MINOR STUDIES FROM THE WELLESLEY COLLEGE PSYCHOLOGICAL LABORATORY

V. The Recognition of Relatively Simple Sensory Experiences

By Edith Brandt Mallory

The problem for which this series of experiments was originally undertaken was to compare the relative recognition-value of concrete materials perceived through different senses. Since complexity was known to be a factor which assists recognition-memory, the material was chosen so that the complexity, both of the experiences themselves and of their associations, would be as nearly as possible equalized, by elimination. Results of tests with these simple units proved so different from what might logically be expected from everyday experience that it seemed desirable to expand the scope of the experiment to discover what would happen with the use of slightly more complex units, and also to investigate the relative *associability* of items in several different sense fields.

Historical. In 1909 Gamble reported a series of Wellesley College memory studies in which smells and colors, as well as nonsense-syllables, were used.[1] The standard test procedure employed was that of the reconstruction method, which was applicable to all three kinds of materials. Her conclusions concern chiefly the differences between the concrete and verbal types, the former being found, on the whole, the easier to learn in short series, and the latter being the easier to learn in long series. A comparison of smells and colors, as materials representative of different senses, does, however, appear in that "neither the numerical results nor the introspective observations reveal any essential difference in memorizing the three kinds of series." This conclusion, it will be noted, concerns the qualitative nature of the processes, and not the relative *ease* of remembering material from different senses. The latter comparison was, of course, not possible in this case, since the various types of material employed were admittedly unequal in respect of the important factors of complexity and interest.

In 1915 Woods made use of concrete material of various sorts, including olfactory, tactual, auditory and visual, as well as verbal items.[2] In this study, however, the aim was not the differentiation of the process of recognition as it occurs in the several types of sensation, but rather an inclusive definition of the experience of familiarity as it is found in all of these various situations.

The direct comparison of memory for the different senses seems to have been

* Accepted for publication September 19, 1932. The experimental work of this study was conducted by Mary Bressler, Mary Butler, Regene Pollock, Rosamond Doering, and Alice Walker.

[1] E. A. McC. Gamble, A study in memorizing various materials by the reconstruction method, *Psychol. Monog.*, 10, 1909, (no. 43), 1-211.

[2] E. L. Woods, An experimental analysis of the process of recognizing, this Journal, 26, 1915, 313-387.

generally ignored, presumably on the ground that really comparable material could not be devised for the different cases; and it must be confessed that the selection of suitable material *is* the paramount difficulty of the problem. The writer's attempts to meet this need are given in detail in a later section.

Gordon studied the influence of complexity on memory by comparing (1) scores for syllables with plain backgrounds with scores for syllables with colored ones, and (2) scores for series of syllables shown all successively in the same place with scores for series in which each item appeared in a different spot.[3] She found that the more complicated stimuli were as a rule the better remembered.

In Peterson's experiments other kinds of variation were used.[4] When memory for words and sentences was compared, the more complex meaning was found to have an advantage in recognition. Series of nonsense syllables varying (1) in size, (2) in color, and (3) in style of type showed marked gains over the plain control series. The influence of size, color, form, and their combinations on memory for plane figures was also noted. For the most part series with combinations of factors were remembered better than simple ones, but since there were only 3 *O*s and individual differences were marked, really clear-cut conclusions were hardly possible.

A general indication of the importance of complexity in recognition-memory appears incidentally in a study by Achilles.[5] Of the three types of materials used—proverbs, words, and nonsense syllables—the most complex kind (proverbs) is found easiest to recognize, and the least complex kind (nonsense-syllables) is found to be the most difficult.

General plan of the experiments. For the two fundamental problems investigated in these studies—(1) the relative recognition-value of simple stimuli in different senses, and (2) the recognition and association values of various combinations of these stimuli—the first step was to devise material which would be uniformly simple, and comparable in all respects except the actual sense avenue through which the *O* was to perceive it. Not only did logical associations need to be equalized or excluded, but other verbal and even merely 'vocalizable' associations as well, since one cannot compare items as representative of their individual sense fields alone if, as in the case of nonsense-syllables, they can simultaneously be imaged in visual, auditory and vocimotor forms, or if they can be adequately differentiated by verbal descriptions which may be imaged in these ways. Such associations were regarded as undesirable because they made it difficult to control the factor of complexity, as well as on account of the fact that they involved sense fields other than the one supposedly represented.

The solution chosen was the use of items which differed from others in a particular series, not by the possession of different qualities, but simply by exhibiting quantitatively different degrees of some one quality. The qualities selected were all of the kind that could be varied along a linear scale: thus items of the visual series differed among themselves only in light-intensity; those of the audltory series, in pitch; those of the tactual series, in roughness; in the thermal series, in grades of warmth and cold; and those in the tests of kinesthetic memory, in extent of passive hand-move-

[3] Kate Gordon, Meaning in memory and attention, *Psychol. Rev.*, 10, 1903, 267-283.

[4] H. A. Peterson, On the influences of complexity and dissimilarity on memory, *Psychol. Monog.*, 12, 1910, (no. 49), 1-86.

[5] E. M. Achilles, Experimental studies in recall and recognition, *Arch. Psychol.*, 6, 1920, (no. 44), 1-80.

ment. With such series the items were too similar to make ordinary associations with concrete memory images useful. If an *O* tried to remember one stimulus as 'like a certain gray dress,' for instance, the other stimuli in the series were also so much like that dress that the auxiliary association did not really distinguish the one item from the other. Neither was it possible to differentiate adequately with verbal descriptions. The terms 'more,' 'less,' 'very much,' 'slightly,' etc., could of course be applied, but these were not effectively definitive, and, whatever their value, would be *equally* possible for *all* of the qualities in *all* of the senses tested.

While, as has been noted above, the individual items could not be distinguished easily by *description*, great care was taken to make sure that the difference between any two items of a series was unquestionably greater than the actual 'difference limen,' *i.e.* the difference was always perceptible when the stimuli were presented either simultaneously or in immediate temporal succession. With the exception of a special case which has been noted, errors may therefore be attributed to the ineffectiveness of memory, and not to inability to make the necessary sensory discrimination.

A number of test methods were seriously considered before the selected ones were adopted. Obviously the usual types of recall tests could not be used uniformly for all the senses studied. Reconstruction would of course be applicable in every case but was ruled out because this procedure does not necessitate memory for the actual sense experience, but merely the retention of the *order* of a series; and this, although perceived through a single particular sense, is apt to be remembered by means of spatial or other patterns, composed of images from other sense fields. These considerations led to the adoption of recognition methods of memory testing. In two of the experiments a simple form was used, a form in which items seen in a previous series are to be recognized and identified when they are again presented together with a number of other items. In one experiment a somewhat more elaborate form was employed in order to test association as well as bare familiarity. Detailed descriptions of both methods are given in later sections of the paper.

The investigation of individual differences, in the abilities tested, would make an interesting sequel to these experiments, but does not come within the scope of this study.

Part I. A Comparison of Recognition-Memory in Different Sense Fields

(Conducted by Mary Bressler)

The purpose of Part I was to compare recognition-memory for materials perceived through four different avenues of sense: vision, audition, touch and the thermal senses.

General procedure. In each of the four parts of this experiment a series of 5 items was first presented, after which came a second series composed of the 5 original items mixed in chance order with 5 others not previously given. As each item in the second series was presented, *O* reported whether she believed it had or had not occurred in the earlier series. Each stimulus lasted for 8 sec., and 20-sec. intervals elapsed between them. In every case *O*'s report was recorded when given, but full introspections were not rendered until the conclusion of the test.

Materials. In the tests of visual memory the material included ten 3 x 3 in. cardboard squares covered with gray papers of 10 different shades, a watch with second hand for timing, and an exposure apparatus. The latter consisted of a screen with

a small square window, behind which there was a carrier for the cards. A spring attachment on this carrier kept the card out of range of the window between presentations, but light manual pressure brought the card readily and quickly into view.

A pitch pipe was used to produce the auditory stimuli. The first series was composed of the notes a, a♯, c, c♯, and e; and these were repeated in the test series combined in chance order with b, d, f♯, g and g♯.

For tactual stimuli, 10 samples of cloth varying in roughness from the smoothness of satin to the coarse texture of burlap were chosen. These were tacked to pieces of wood in a way to insure even weight and hardness to the surfaces.

Supplies of ice and boiling water, as well as of plain tap-water, were provided in the preparation of thermal stimuli. By the combination of these, water was mixed in cups to the temperatures: 10°, 15°, 20°, 30°, and 40°C., for one series, and to 5°, 25°, 35°, 45°, and 50°C., in addition to the above for the second series. A standard cup of water at 26°C. was also maintained (see below). These temperatures were checked and tested with a thermometer at the beginning of each stimulation.

Directions to observers. The following directions, in type-written form, were handed to the *O*s before each part of the experiment.

"You will have presented to you a series of stimuli. Each stimulus will last 8 sec., and there will be 20 sec. between presentations.

"Immediately following the first series you will have the same stimuli presented to you, mixed with new 'confusion' stimuli, and you are to recognize as many as possible of those that were in the first series. After each stimulus in this second series, say 'yes' or 'no' to show that you do or do not recognize it as one of the earlier series. You may add 'doubtful' if you are not sure of your decision."

The *O*s were instructed which type of stimuli, whether visual, auditory, tactual, or thermal, was to be given, in each case.

Methods adopted to avoid error. Care was taken that the *O*s should receive no cues from seeing the *E* prepare or adjust the material. During the tests of visual and of auditory memory, all necessary manipulation of gray cards and of the pitch pipe was concealed from the *O*s behind a cardboard screen. In those parts of the experiment involving tactual and thermal stimuli the *O* was blind-folded.

In order that the *O*s should be passive in their perception of tactual and thermal stimuli as well as in vision and audition, special technique was employed in the two former cases. The *O* did not actively rub the cloth, but simply laid her hand on the table, palm up, with middle and index fingers extended toward *E*, while the latter drew the cloth-covered blocks of wood steadily and evenly across the tips of *O*'s outstretched fingers. In the case of thermal stimulation, *O*'s fingers were guided to the cup and dipped into the water (always to the same depth) by *E*.

Under the given conditions of the experiment *sensory adaptation* offered no particular difficulty in the case of vision and audition, but in the other two tests some care had to be exercised in this connection. The *O*s were asked to report any numbness or confusing sensations which may have occurred in their work with the different grades of roughness and were told to rub the fingertips on the palm of the hand in the intervals. (Very little difficulty or confusion from this source was reported.) Adaptation to temperature proved to be a much more serious problem. Before every regular item in the thermal series *O*'s fingers were placed in a cup of water of standard (26°C.) temperature, and then dried. In this manner it was assured that the skin would be always at the same temperature when the stimulation

was given, so that the stimuli when repeated in the second series would feel exactly as they did in the first, because of the same relative sensory difference as well as because of the same absolute degree of heat.

Observers. Of the 20 *Os* who took part in this experiment 17 were students, and three were members of the college staff in psychology.

Scoring. In any test where the answer can be only one of two choices, it is possible for a half of the perfect score (in this case 5) to be made by sheer chance. The score for each test was, therefore, the number of items in the test series reported correctly, minus 5. The score for each type of sensory material was determined by averaging the scores of all 20 *Os* for each class of test.

Results. The average recognition score for grays was 0.60, P.E. = 1.27; for pitch, 0.75, P.E. = 1.1; for roughness, 1.45, P.E. = 1.1; and for temperature, 1.75, P.E. = 0.84.

The striking fact here is that although all the items were easily susceptible of immediate sensory discrimination, and there were only 5 items to be remembered, the scores are all so low as to be possible by the operation of mere chance.

The slight differences which occur show higher scores for tactual and thermal memory than for visual and auditory memory, an outcome which appears at first glance extraordinary, since it is a commonplace of experience that we use and rely on memory in the two latter fields rather than in the two former ones. Reflection suggests that it may be the wealth of associations which are usually combined in our auditory and visual experiences which make them superior in everyday life, but this does not in any way account for their making a *lower* score than tactual and thermal stimulation in the laboratory situation. Suspecting that the answer might lie in some unanticipated peculiarity either in materials or in procedure, *E* reëxamined these with care. A study of the *Os'* introspections brought to light the fact that the *texture of the fabrics* had afforded composite experiences of softness, irregularity, clingingness, etc., as well as roughness, and occasionally even the identifying name of the fabric. Obviously then, the tactual stimuli had not been 'simple,' as intended, but really possessed very complex qualities; and thus it seemed more probable that the superiority in memory-value was due to complexity rather than to the sense modality. When the various *procedures* were compared it was found that the experiment with temperature differed in one respect from all the rest. In order to rule out complications arising from sensory adaptation, a standard stimulus of 26°C. was given before each regular stimulus. In so doing, however, *E* had inadvertently presented a much more complex situation in this test than in the others. Two related items, instead of one, had really been presented in each case. Evidently the thermal stimuli, as well as the tactual ones, were too complex to be judged on even terms with those of vision or audition, since in these instances the factor of complexity (to be studied in the following experiment) was in operation.

Conclusions. (1) Recognition memory for material of the very simple type used in this experiment is exceedingly difficult. *(2)* The apparent advantage of tactual and thermal material is probably due to the fact that these were more complex than the other two types.

PART II. THE RECOGNITION VALUE OF COMPLEXITY IN MATERIAL AS PRODUCED BY THE COMBINATION OF SEPARATE STIMULI

(Conducted by Mary Butler and Regene Pollock)

Part II was devised to discover the effect of complexity of material on recognition-memory.

General procedure. Material from three senses was used—vision, audition, and touch. The materials were gray papers, tones of different pitches, and grades of roughness as found in a series of sandpapers. Tests were given for various combinations of these materials, *i.e.* for series of (1) single stimuli in each sense field, (2) pairs of stimuli in each sense field, (3) pairs made up of combinations of material from two sense fields, and (4) trios combined from all three senses. The simple recognition test, described in the preceding experiment, was used. Stimuli lasted for 2-sec., and 3-sec. intervals elapsed between them.

Materials. For the tests of visual material, cardboard squares of varying shades of gray were used. These were made by dyeing white cardboard with graded solutions of black ink. They were chosen as approximately equidistant one from another along the range from very light to very dark gray and were discriminable at a distance of 30 ft. A purely subjective criterion (agreement of 3 *O*s) was used in selecting these shades, but subsequent testing by means of a Nicol prism polariscope showed that each gray had between 80% and 85% of the intensity of the next brighter stimulus. For audition, a pitch pipe was used as in the preceding experiperiment. Ten pieces of sandpaper of discriminable grades of roughness were used as touch stimuli. These were glued to blocks of wood to insure equal weight and hardness of surface.

Directions to observers. Typewritten copies of the instructions were handed to *O*, and at the same time read aloud by *E*. Sample directions of each general type are given below.

(a) "I am going to present to you a series of 5 grays differing in degrees of brightness. I want you to try your best to remember these, so that you will be able to recognize each one when you see it again. After I have shown you the first series, I will present a second series composed of 10 grays, which will include the 5 that you have just seen, as well as others. Try your best to identify those first 5 grays."

(b) "I am going to give you a series composed of 5 somewhat complex situations, each affecting two senses; namely, touch and hearing. Each will be composed of a certain degree of roughness and a certain degree of pitch. After I have shown you the first series," etc.

(c) "Now I am going to give you another series to remember. This time both stimuli of each pair will be touch sensations, of particular degrees of roughness," etc.

(d) "Now I am going to give you a series of experiences in which three senses are stimulated," etc.

As the second test series was shown *O* responded to each item or combination of items with 'Yes' or 'No,' according as she did or did not recognize it as one of the first series.

Methods adopted to avoid error. Care was taken to be sure that all the selected grades or degrees of each quality used were discriminable one from the other when experienced simultaneously or in immediate succession.

The visual exposure apparatus was covered with green so that no relationships could be readily formed between it and the grays.

In order to prevent any visual aid in the tactual part of the experiment, *O* was made to extend her hand through an opening in a cardboard screen, and the application of the sandpaper was thus entirely concealed from her sight.

The pitch pipe was also used behind a screen, out of sight of *O*.

To avoid giving any particular material the advantage of practice at the end of the experimental period, or of initial zeal at its beginning, one *O* started with test No. 1, the second *O* with test No. 2, and so on. As they continued through the entire series, such rotation equated these factors for all the kinds of material.

Observers. The *O*s of this experiment were 37 members of Wellesley College, most of them students. The first seven sets of tests were regarded as a preliminary practice experiment, in the course of which the *E*s improved and mastered their technique. The results given below are derived from the scores of the last 30 *O*s.

Scoring. In this study the tests were scored by the 'right-minus-wrong' formula to eliminate the chance of scoring by mere guessing; that is to say, the number

TABLE I

SHOWING SCORES FOR DIFFERENT COMBINATIONS OF MATERIALS

Average Scores for	Vision	Audition	Touch	Scores for different degrees of complexity	
				Average	P.E.
Simple items.....	2.86	1.63	2.73	2.24	.84
Pairs in single sense..	4.53	3.40	2.60	3.49	.95
Pairs in unlike senses		3.66			
			3.86		
		4.86		4.29	1.08
All tests in which a particular kind of material occurred	4.3	3.66	4.03		
Trios of all three senses combined:			5.68	5.68	.79

of wrong reports was subtracted from the number of right reports in each test. The scores given as representative of each type of material are averages of the tests of 30 *O*s.

Results. Table I shows the average scores for tests involving single items in each sense field, pairs of stimuli from the same sense, pairs from dissimilar senses, and trios.

Scores for the single items (2.86 for vision, 1.63 for audition, and 2.73 for touch) show again that touch in its simple form is not harder to remember than items from the other two senses. Audition appears exceedingly low. This result, which seems strange in view of our extensive use of auditory memory for the complex sorts of stimuli usual in everyday life, is corroborated by the acknowledged rareness of the occurrence of 'absolute pitch' where memory for single tones is required.

Scores for pairs show a different tendency. The visual pairs gave an average score of 4.53, auditory of 3.4, and touch of 2.6. Whereas in the first two senses pairs were almost twice as easy to remember as the single items, touch showed no advan-

tage in scores for the pair as compared with the single unit. Either two touches are not easily associable one with another, or sensory adaptation interferes with discrimination when several sensations of roughness occur in succession. The following scores for dissimilar pairs, however, seem to show that touch is of value when associated with material from another sense. These data will support the view that sensations of roughness are associated as readily as any other sensation if adequately perceived, but, whereas they are adequately perceived one at a time, sensory adaptation confuses the experience if stimuli occur in too close succession. Vision and audition combined received a score of 3.66; audition and touch, 3.86; vision and touch, 4.86; and the trio (vision, touch, and audition), 5.68.

When averages were taken for all of the tests in which vision was involved, vision had a score of 4.30, audition a score of 3.66, and touch a score of 4.03. It would appear that touch is of as much assistance as any other sense as a supplementary factor, although two touch stimuli do not make a combination that is easily remembered. The striking influence of complexity is shown in the following comparisons. When scores for all the single items are averaged, their score is 2.24, P.E. = 0.84; the average for all pairs is 3.89, P.E. = 1.02; and the average for the trios is 5.68, P.E. = 1.26.

Conclusions. (1) In general, the results of this experiment confirm those of the first one in that variations in memory-value of different sense materials are slight. Audition, however, is poorer than vision or touch when represented by simple stimuli and roughness is lower than the others when represented by complex stimuli. *(2)* Touch stands out from the other two senses studied in that, whereas it assists memory when combined with other senses, two touch stimuli appear to be no easier to remember than one touch stimulus. Sensory adaptation may be responsible for this apparent discrepancy. *(3)* Materials from different sense fields can be combined, and such combinations are as easy or easier to remember than combinations of stimuli from the same sense field. *(4)* Real differences show themselves between the memory-value scores for simple and complex material; the more complex the stimulus the higher the score.

Part III. The Associability of Stimuli from Different Sense Fields

(Conducted by Rosamond Doering and Alice Walker)

The problem investigated in Part III was the associability of stimuli perceived through three different sense avenues; namely, touch, vision, and kinesthesis. Since in the preceding experiment simple elements showed approximately the same memory values, regardless of the senses represented, and since the more complex combinations were remembered better than the simpler ones, it was surmised that the advantage usually accredited to visual (and auditory) memory in everyday experience might be due to such complexity. There arose therefore the question whether percepts received through certain senses were more complex merely because these sensory systems admitted a greater wealth of data, or because experiences received through such senses were especially easy to associate and therefore to organize.

General procedure. The associability of different materials was tested by the following method. There was first presented a series of 8 items, grouped in 4 pairs. *O* was then given the first member of some one of these pairs, and asked to select,

out of three stimuli offered, the second member of that pair. Memory for the other three associated pairs was similarly tested. The technique might be described as a modification of the method of right associates, in which a multiple-choice recognition test is substituted for the usual recall test. There were 9 tests in each complete cycle, arranged to test the associations of (1) grays with grays, (2) length of passive hand-movement (which, for the sake of brevity will be called "distances") with other distances, (3) grades of tactual roughness with other grades of roughness, (4) gray with roughness, (5) roughness with gray, (6) gray with distance, (7) distance with gray, (8) roughness with distance, and (9) distance with roughness. A 2-sec. exposure was allowed for each stimulus, a 3-sec. interval was allowed between the pairs, and a 5-sec. interval elapsed between the first presentation and the test series.

Materials. The visual items were 2-in. squares of Hering gray papers, mounted on cardboard. They were presented in the exposure apparatus described in Part I.

For tactual stimuli there were used a series of 10 grades of sandpaper varying serially from very rough to very smooth. These were mounted on blocks of wood to equalize their hardness and weight.

In the tests involving passive arm-movements there was used a special apparatus designed by Professor M. J. Zigler. On a heavy wooden base is a well-oiled metal groove with a millimeter scale. The carrier which slides along this groove has two knobs or handles so that with one it can be manipulated by *E* while *O*'s hand, grasping the other handle, is moved the required distance. This apparatus was placed so that all motion was away from and parallel to the median plane of *O*, and in line with her shoulder.

A large table screen with curtained aperture to admit *O*'s arm concealed from her sight all procedure of the touch and 'distance' tests.

An exposure apparatus for grays was placed at the side of the screen, where it could conveniently be observed. A metronome was used for timing.

Directions to observers. When *O* was seated at the table in front of the screen *E* announced:

"In the course of this experiment you will have stimuli of various sorts, presented in pairs. They will include certain shades of gray, certain degrees of roughness, and certain distances that your hand will be moved. I will give you first a pair made up of two shades of gray [or whatever material was scheduled for the beginning of the test], then other pairs of grays until I have given you four pairs. I want you to associate the two grays in each pair together in such a way that when I later give you the first member of the pairs you will be able to pick out, from a group of three, the other gray which has been presented. Is this clear?"

Questions were answered and preliminary trials were made until it was certain that *O* understood exactly what was required. Similar forms of directions were used for the other materials and for their various combinations. None of the grays were repeated in the course of any one test.

Methods adopted to avoid error. A certain number of tests were given in which the four initial members of the pairs were shown in the same order in which they had originally been given, but it was discovered that under these conditions the *O*s were not really associating the two members of each pair, but rather remembering the pattern of the entire series of eight and trying to choose the stimulus that they recalled as "number 2" or "number 6." A change was therefore made in the technique, and the initial members of the pairs were presented in the test series in an order

different from that in which they had at first appeared. Only the scores from the revised form of test are reported; the others were discarded as irrelevant to the problem in hand.

Observers. Six *O*s, five undergraduates and one member of the laboratory staff, took part in this experiment. All were trained, and accustomed to serving in psychological experiments.

Scoring. The score for each test was the number of pairs correctly associated. The highest possible score would thus be 4, and a score of approximately 1.3 would be expected from the operation of sheer chance. Each *O* took from two to eight cycles of tests, so that the final score reported for each of the nine combinations of materials is an average of the scores of 40 tests.

Results. Table II gives the averages found for the various combinations of materials.

For pairs of items from the same sense field, grays had an average of 2.62; roughness, 2.23; and distance, 2.91; the average for all like pairs was 2.65. Roughness pairs are below the others. For unlike pairs the averages were 2.31 for gray and

TABLE II

SHOWING SCORES OF RESULTS OF TESTS OF ASSOCIATION BETWEEN DIFFERENT KINDS OF MATERIALS

(The materials listed in the first column are those given as first members of pairs; those in the row across the top were given as second members of the pairs.)

	Gray	Roughness	Distance	Av.
Gray	2.6	2.3	2.8	2.6
Roughness	2.7	2.2	2.9	2.6
Distance	2.7	2.3	2.9	2.6
Av.	2.7	2.3	2.9	

roughness; 2.77 for roughness and gray; 2.87 for gray and distance; 2.77 for distance and gray; 2.99 for roughness and distance; and 2.32 for distance and roughness. An average of 2.68 for all unlike pairs was found.

The similarity of these figures suggests very strongly that pairs of simple stimuli can be associated with about equal facility, regardless of which of the three senses they involve. Slight differences were found between the senses when averages were taken for all the pairs containing each type. All the pairs containing gray gave an average of 2.67; the average of all those containing roughness gave 2.53; and the average for all combinations containing distance gave 2.78. When averages were scheduled for all the tests in which each type of material was used as the first member of the pairs, *i.e.* as 'cues,' scores for the three types were practically identical: viz. gray, 2.60; roughness, 2.67; and distance, 2.67. When these materials were used as the second members of the pairs, that is, the ones that had to be selected as familiar from groups of three, a little difference was found. The score for gray was 2.72; roughness, 2.29; and distance 2.92. Here, as in Part II, perception was probably at fault in the case of roughness.

Conclusions. (1) Simple experiences perceived through the three senses studied—vision, touch and kinesthesis—appear to be equally easy to associate. *(2)* It is as easy to associate two items from different senses as it is to associate two items from the same senses. *(3)* The apparent slight inferiority of memory for the connection of two

items both in the field of touch may be due to difficulties attending their perception rather than their association.

General Discussion

Since the results of the foregoing studies do not demonstrate those differences in memory-value that are usually ascribed to the various senses (and which are indeed very regularly found in introspective reports of memory-imagery), there arises the question: What is the difference between these experimental conditions and those of everyday life? In the latter practical situation the experiences ordinarily differ from these laboratory stimuli in their more varied range of interest, complexity, and organization. In the opinion of the writer the apparent advantage of memory for some senses is actually due to these factors, to which both the wider range of discriminable qualities and better opportunities for relationship (such as the spatial connections of vision and the temporal aspect of audition) especially contribute. Hollingworth has made interesting use of these points in his discussion of the value and relationships of the senses.[6]

In view of the fact that the differences (in pitch, light, intensity, roughness, or temperature) between the items in any given series were all greater than the *Os'* limen for immediate sensory discrimination, and that only five items were presented in each original series, the recognition scores, in some cases only slightly above what might be expected by pure chance, seem astonishingly low. The obvious interpretation would appear to be that the extremely low scores are due to the extreme simplicity of the stimuli. If this be true, it suggests that complexity may be thought of not merely as an 'aid' in recognizing, but as fundamentally necessary to the process. Completely simple and unrelated items cannot, presumably, ever be given, since temporal and spatial as well as other self-relating associations are bound to enter in,[7] but as the connections are reduced the possible memory value seems to become slighter and slighter. If one may argue from the analogy of the variable approaching a limit, one might surmise that a completely unrelated homogeneous stimulus must be completely impossible to remember.

Summary and Conclusions

It was noted that, in general, an individual remembers experiences received through certain senses more frequently and more accurately than he remembers those perceived through certain others. There arose, therefore, the question whether this difference is to be attributed to the fact that it is actually easier to remember material for one sense than it is to remember equivalent material from another, or simply because in the former case there is commonly some special characteristic such as variety, complexity, attention-value, etc., which gives it superior memory-value.

In order to make the material as nearly as possible equivalent for the several senses, the factors listed above were ruled out by selecting in each sense material that varied only in the quantitative degree to which it possessed some single quality—*e.g.* roughness in touch, and reflected light-intensity in vision.

These very simple stimuli and also slightly more complex ones were used in

[6] H. L. Hollingworth, *Psychology: Its Facts and Principles*, 1928, 459.

[7] Cf. E. G. Boring's description of consciousness as relational, which has appeared since the acceptance of this article for publication. *The Physical Dimensions of Consciousness*, 1933, 222-229.

recognition tests of the 'identification' type. Tests to measure the ease of associating the simple sensory items were also given.

Results of these studies have led to the following conclusions:

(*1*) None of the senses studied shows any marked superiority over the others in the recognition-value of its simple elements.

(*2*) Simple items in vision, kinesthesis and touch appear equally easy to associate, although difficulties of sensory discrimination give an apparent disadvantage to the association of one touch stimulus with another.

(*3*) Complexity is of great importance as an aid to recognition value. It appears possible that the differences in memory-value customarily attributed to the several senses may be due largely to variations in this factor.

(*4*) It is possible to devise materials representative of several senses, that are comparable to each other in respect to the factor of complexity, by using series in which the items vary only by the possession of different quantitative degrees of some single quality.

APPARATUS

THE TACHISTOSCOPE AS A MEASURE OF READING PERCEPTION

By FRANCIS P. ROBINSON, University of Iowa

Both tachistoscopic and reading perception are characterized by a limit to the amount of material that is perceived. Because of this, one has been assumed to be a measure of the other. Thus, Tinker,[1] noting that several investigators have found that perception-span correlates fairly well (0.50–0.70) with the ability to score on reading tests, concludes that "the span of visual apprehension for certain types of material is an important determinant of reading ability." Brooks interprets the tachistoscopic span as being the physiological limit determining the upper levels of reading-span development.[2] The purpose of this study is to evaluate the tachistoscope as a measure of this aspect of reading perception.

The experimental procedure for determining the relationship of these two variables was as follows. The eye-movements of 51 college students were photographed as they read a given selection. Their average width of fixation was found in terms of words by dividing the number of fixations made into the total number of words read. Following accepted scoring procedure, the first line was omitted from these computations. Their perception-span was found by taking the average number of words perceived in 30 tachistoscopic presentations of unrelated sentences. Tinker's method of scoring one point for each word right and one-half point for words in the wrong order was used.[3] The nature of this tachistoscope will be described later.

In order to allow the most reliable determination of these spans, the following controls were set up for this experiment. A very simple story, that of Aladdin, was used so as to eliminate difficulties in vocabulary and comprehension. Since this reduced regressions to a minimum, it facilitated the computation of a valid width of fixation. It also allowed the determination of the maximal perception-span unaffected by long or hard words. Several trials were given before each part of the experiment to adjust the student to the experimental situation. The stimuli for each experiment for any one individual were selected from different parts of the story, but by rotating these parts with each new case, the same stimuli were used by the group in all situations. It had originally been intended to restrict the computation of the average width of fixation to the same lines that were used in the

[1] M. A. Tinker, Visual apprehension and perception in reading, *Psychol. Bull.*, 26, 1929, 223-240.
[2] F. D. Brooks, *The Applied Psychology of Reading*, 1926, 37.
[3] M. A. Tinker, The effect of color on visual apprehension and perception, *Genet. Psychol. Monog.*, 11, 1932, 61-136.

tachistoscopic test, but when this was found to correlate 0.95 with the average reading-span for the whole selection, the latter was used because of its greater reliability.

The group's average width of reading fixation was 1.64 words, S.D. 0.43, while their average perception-span was 3.39 words, S.D. 0.76. It will be noted that according to Brook's contention, the width of fixation is less than one-half of what it might be, but studies of superior readers[4] indicate that their reading-span never does approximate such a value and studies in training indicate that they cannot be made to do so.[5]

The point of interest here is, however, the actual relationship between these two variables. The correlation found was $0.52 \pm .07$ which is substantiated by the correlation of $0.46 \pm .06$ found by Litterer.[6] The higher correlation in this experiment, if there is a true difference, may be attributed to the use of identical materials and similar procedures in the two situations which Litterer did not have, or possibly to a slightly greater range of reading ability. It might also be noted here that as an aspect of an earlier study[7] the writer correlated these two variables using a small group of a very restricted range of reading ability. The correlation found was very small, but the discrepancy is explainable in terms of the range of reading ability measured. In fact that experiment led to this study.

These results indicate that perception-span as measured by the usual tachistoscopic procedure is somewhat related to reading span but that such a perception-span probably should not be used for predicting what the reading span is, or what it should be.

In order to determine if the use of a tachistoscope which was more like the reading situation would raise this correlation, the tachistoscopic procedure was changed in the following manner. Since, in reading, the theme of the story indicates something of what is to come, in this tachistoscopic experiment the sentences were presented in their meaningful order. To present these sentences one at a time would not, however, duplicate the reading situation because the long waits between exposures would allow the meaning to drop out and the psychological set would not be like that in reading. So in this study a tachistoscope was built which allowed *O* to read a paragraph down to the last line which was then exposed tachistoscopically for 100 σ by his pressing a key (see Fig. 1). It will be noted that the exposure of the sentence after the return sweep—which is the most pronounced break in the time sequence of reading—introduced as little distraction as possible into the reading. The students were given several trials to introduce them to this coordination and they had already used the apparatus for exposing single sentences in the other experiment on perception.

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[4] R. Y. Walker, The eye-movements of good readers, *Psychol. Monog.*, 44, 1933, (no. 201), 95-117.

[5] C. C. Ring and M. Bentley, The effect of training upon the rate of adult reading, this JOURNAL, 42, 1930, 429-430.
F. P. Robinson, The rôle of eye-movements in reading with an evaluation of techniques for their improvement, *Univ. of Iowa Studies: Series on Aims and Progress of Research*, No. 39, 1933, 1-52.

[6] O. F. Litterer, An experimental analysis of reading performance, *J. Exper. Educ.*, 1, 1932, 28-33.

[7] F. P. Robinson, *op. cit.*, 35-37.

This apparatus is like the reading situation in several other respects. The material appears as a book-like surface-plane at an efficient angle for reading; the visibility of the moving part is minimized by painting it the color of the paper and screening it as much as possible; the lighting comes from an ordinary reading lamp in a conventional position for reading; and the *O* is unrestricted as to his sitting position.

The controls used to make the two tachistoscopic and eye-movement situations comparable, besides those already described, were as follows. Not only was all of

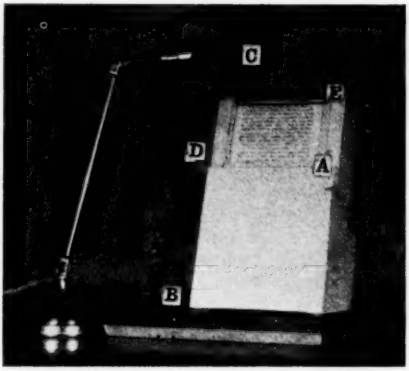

FIG. 1. A TACHISTOSCOPE FOR THE STUDY OF READING

It will be noted that this exposure device allows the reading of the printed material (as much as 20 lines) down to the shield (A) which covers one line. When *O* reaches the end of the last visible line and jumps back to where the first of the next line will begin, he presses the key (B) which causes the covered line to be exposed for 100 σ. The pressing of this key releases an unbalanced rocker-arm switch which has contacts on either end. During part of the swing of the falling arm, both contacts are out of their mercury cups which breaks the circuit to the magnets that hold the exposure arm up. The distance of the fall of this rocker-arm determines the interval of exposure. Thus the time interval can be changed by raising or lowering the mercury cups. The P.E. of such a timing device is about 4 σ. The reading lamp (C) illuminated the material and the mirror (D) is used for checking the location of the reading material as it is rolled up on roller (E) after each exposure. To obtain the usual tachistoscopic set-up, single sentences were exposed behind this arm without any preceding reading matter.

the same story used in each situation through rotation, but in the two tachistoscopic tests the same sentences were used. Since the last line of each paragraph had been written to stand grammatically alone as a sentence, these were taken and placed in an unrelated order for the first tachistoscopic experiment. Thus any difference found in the spans for the two tachistoscopic situations can be attributed to the meaning added by the previous story.

The group's average perception-span for this modified tachistoscope was 3.97 words S.D. 0.81, for 30 presentations. This is 17% greater than their span on the

conventional tachistoscope and represents a reliable difference (10.51 times the the S.E. diff.) The intercorrelation of these two perception-spans is 0.88 (0.91 attenuated). Two points of interest are indicated by these results. First, the basic perception span, as it has been related to reading, is larger than it is usually described. Thus the average reading span of this group represents only 41% of their perception-span. Secondly, it might be noted parenthetically that since the added reading material in this tachistoscopic situation has resulted in an increased span, the difference found might be taken as an objective measure of the meaning of the previous story. That is, it would be a measure of the meaning as it contributes to the idea of the exposed sentence.

The point of particular interest here, however, is the relationship of this perception-span to the average width of reading fixation. The correlation was $0.66 \pm .05$. This is 0.14 higher than the correlation between the usual tachistoscopic and reading spans and represents a reliable difference (4.07 times the P.E. diff.). Thus the alteration of the tachistoscopic procedure into one more closely approximating the reading situation produces a better measure of reading perception than the usual tachistoscopic procedure, but even yet it does not produce a good measure of what the reading span is or even what it should be. Furthermore, the interrelationship of these spans is not sufficient to explain the correlations found between perception-span and reading ability. Other factors, such an intelligence and motivation, are probably also important.

The conclusions of this study are as follows. First, the usual tachistoscopic test is not a very good measure of the average span used in reading ($r = 0.52$) nor probably of the limits of its development. Secondly, the technique developed in this study for measuring perception-span after having read a paragraph of meaningful material is a reliably better measure of the reading span (4.07 times the P.E. diff.), but it still is not a very good predictor of eye-movement behavior ($r = 0.66$). Since most other measures, however, do not correlate very highly with reading ability, these results indicate that the technique described here has some value for the analysis of reading ability. The feasibility of using this approach to analyze the accuracy of reading perception has yet to be evaluated.

AN IMPROVED RESETTING CHRONOSCOPE

By SAMUEL RENSHAW and I. L. HAMPTON, Ohio State University

The first magnetic resetting chronoscope employing a new type of clutch was designed and built in 1929 and was shown by us at the Cornell meeting of the American Psychological Association in September, 1932. A description of the first model of the non-resetting instrument was printed in this JOURNAL.[1] Certain improvements, including the improved reset mechanism and its circuit, seem to warrant this brief description.

[1] A combined chronoscope and interval-timer, *op. cit.*, 43, 1931, 637 f.

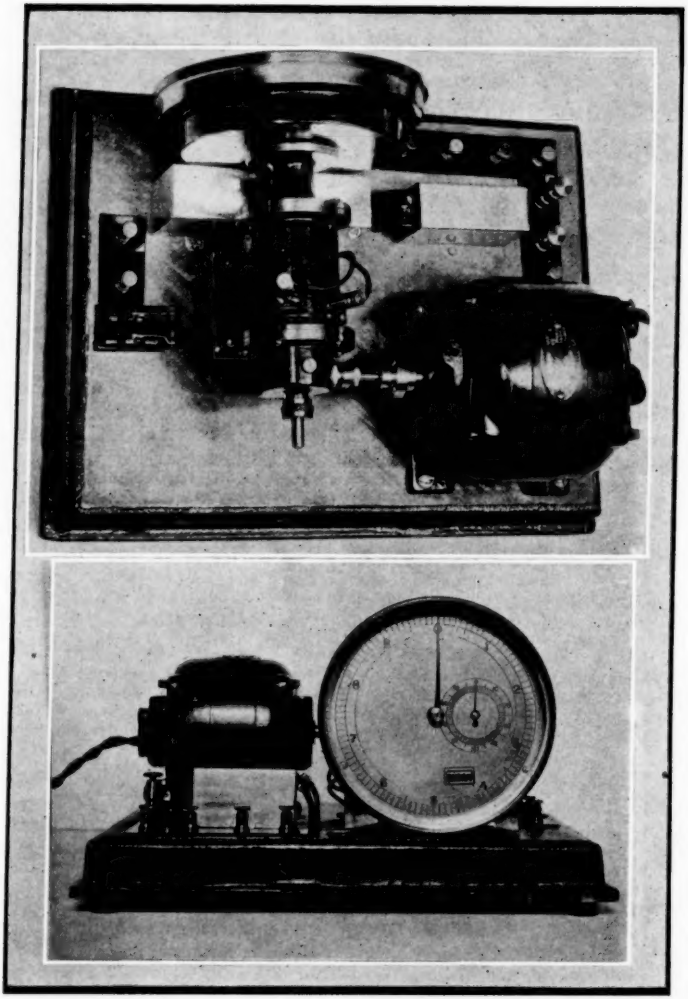

FIG. 2. AN IMPROVED RESETTING CHRONOSCOPE

This instrument possesses several features which are improvements on most existing chronoscopes, and which increase its usefulness in many experiments where accurate timing is desired.

The dial is large and easily read. Seconds, hundredths-seconds, or sigmas may be read off directly, as one pleases. Times from sigmas to hours can be measured with great accuracy. The dial is enclosed in a dust-tight housing with a beveled plate glass cover. The non-resetting Veeder counter provides for the immediate recording of times greater than one second. Pressure on the reset button, located just below the dial to the left, resets to 0. Resetting can be done also by remote control by the use of a pair of binding posts in parallel with the reset push button. Cumulative timing useful in certain kinds of experiments is accomplished by a single make-break key.

The clutch engages at any point in its circumference. There are no teeth to mesh. The holding power of the clutch is several hundred times the actual demand for moving the index hands. From this detail we derive a high degree of constancy. One hundred consecutive timings of a contact-pendulum falling through 20° of arc gave the following distribution:

Time (sec.)	n
.169	1
.168	8
.167	32
.166	41
.165	15
.164	3
Total	100
Mean	.1663
S.D.	.00095

The instrument has few moving parts and cannot be tinkered into a new set of error constants. When it is once calibrated, the only variable is the constancy of the cycles of the 110-volt A.C. current supplied to the synchronous motor. In most modern power plants this is held to a very low range of variation. The accuracy of timing with the instrument depends upon the equalization of the starting and stopping lags. The former is determined by the time required to build up the magnetic field in the clutch coil and the latter is determined by the energy stored in the music wire spring of the rotating member of the clutch. Since this moves only 0.0025 in. in engaging it is possible to balance these lags very closely and any discrepancy can be detected by calibration of the constant error, which then can be added or subtracted to the readings. All construction of moving parts is to precision standards. Occasional oiling is the only attention needed. The whole mechanism operates with very little noise, the A.C. hum of the motor being the largest contributor. The resetting through an external key makes possible mounting the entire instrument in a sound proof box with a glass window in those experiments in which complete silence is required.

The interval timer can be attached as a separate unit to the main drive shaft which is seen extended from the reducer unit, at the rear of the clutch. A chuck mounted on this shaft, carrying a commutator disk with suitable contacts, will give

a convenient and readily accessible source of seconds or fractions of one second. A simple stepping unit of the star wheel type can be made to provide for the control of much larger temporal intervals. A light-chopping disk can also be mounted on the main shaft and a photocell circuit used in those experiments in which such apparatus is desired.

The cost of the unit complete is no greater than the most widely used similar instrument, and its superiority has been clearly demonstrated in our laboratory.

A 6-volt D.C. current operates the clutch and make-break circuits. Two binding posts, shown at the right of the dial, are in series with the clutch coil. Thus in a reaction-experiment the stimulus can be delivered simultaneously with the engaging of the clutch. A holding relay is energized by the *E*'s key so that the *E*'s reaction time is entirely eliminated. The resetting is done by the clutch itself. When the reset key is depressed the clutch coil is energized through a special holding relay which returns the index hands to the vicinity of the zero point. Here the circuit is mechanically broken and a magnet is energized which forces its armature pawl into the hilum of a cordate cam which completes the return of the index hands to the exact zero position. Resetting occupies on the average about 0.5 second. The essential units are mounted on an iron base casting 10 x 13 in., resting on rubber feet. This casting is faced with a plate of polished non-rusting, non-corroding Enduro iron.

A SWITCH FOR THE PRESENTATION OF ELECTRICALLY GENERATED TONAL STIMULI

By S. S. Stevens, Harvard University

In the presentation of electrically generated tonal stimuli it is frequently of great importance that the click which accompanies an instantaneous 'make' or 'break' of the circuit be eliminated. This click can be suppressed if the potential across the receiver is changed gradually and not abruptly both at the beginning and at the end of the presentation.

The device shown in Fig. 3 provides a convenient means of presenting 'clickless' tones. It consists of a two-pole, double-throw switch to the terminals of which are fastened brass extensions. These extensions make contact with the blades of the arm of the switch and maintain that contact throughout all but a small sector of the arc through which the arm swings. A string runs from the arm over a small pulley, situated directly above the 'off' position of the switch, to a pin fastened in the edge of a gear of 2½-in. diam. This gear drives a small gear, 1-in. diam., fixed on a shaft, on the other end of which is mounted a small wire-wound, variable resistance. This resistance is permanently connected across the receiver so that when its value is minimal the current is shunted through it and does not go through the receiver. Opposite the pin, in the larger gear to which the string is fastened, is another pin to which are attached enough rubber bands to pull the resistance back to its 'off' position whenever the tension on the string is relaxed.

The operation is simple. When the arm of the switch is in the vertical position,

between the two pairs of brass extensions, the circuit is open and the resistance is low. As the arm moves to the right or left (depending on which tone is to be presented first), it makes contact with the brass extensions and closes the circuit for one of the tones. Since the resistance is still low, however, the receiver is short-circuited, and is not activated. Then, as the arm moves farther, the string begins to turn the gears and the variable resistance, introducing resistance into the shunt across the receiver, and thus increasing the amount of current through the receiver. The tone is terminated by the reverse process; and the second tone can be turned on and off in a similar manner by the other half of the switch.

This switching device has been used in the output circuit of an oscillator in an experiment in which two tones were presented alternately at the rate of about 40

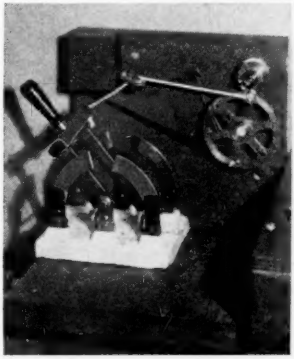

FIG. 3. SWITCH FOR THE PRESENTATION OF ELECTRICALLY GENERATED TONAL STIMULI

per min. At this rate of operation the variable resistance requires about 0.2 sec. to make a complete excursion from the 'off' to the 'on' position. Under these conditions the click is effectively eliminated.

The magnitude of the variable resistance used should be determined by the characteristics of the circuit and of the receiver or loud speaker. In the experiment mentioned a resistance of 5000 ohms was used in connection with a Western Electric earphone and a resistance of 2000 ohms with a pair of Jenson loud speakers. The effectiveness of the device could be somewhat improved if a variable resistance wound on a logarithmic form were used. With this type of winding the resistance is increased slowly at the beginning, but rapidly towards the end of the excursion of the switch.

A MICRO-RECORDER ADAPTER FOR USE WITH KYMOGRAPHS AND POLYGRAPHS

By Edward W. Geldreich and Theodore W. Wood,
University of Cincinnati

Investigators who have had experience with various recording devices, viz., tambours and other markers, have found difficulty in causing the styluses of such instruments to make proper contact with the paper of kymographs and polygraphs.

Recording instruments have been designed to eliminate a part of this difficulty. Such designs usually operate with a cog gear that provides a horizontal or parallel displacement or approach of the recording device to the plane of the paper on the

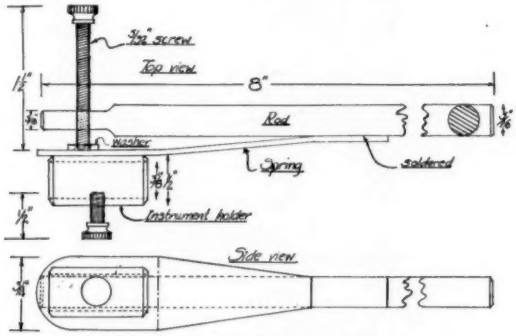

Fig. 4. Diagram of the Micro-Recorder Adapter

kymograph or polygraph, but they do not provide a perpendicular displacement or approach of the stylus to the plane of the paper. Obviously, in actual operation, this limitation involves first the lining up all the recording instruments parallel to the axis of the drum of the kymograph or polygraph, and then the moving of each instrument to the drum, when it is often discovered that the whole ringstand must be set over to secure the optimum angle for contact of the stylus and operation. There is also the necessity for frequent and delicate adjustment while records are being made.

An instrument designed by the authors eliminates this difficulty by causing the stylus of any recording device to approach the recording paper slowly and to make fine contact with it. The essentials and dimensions of this device are shown in Fig. 4. One feature of the construction must be carefully observed: the hole through the brass block, which is to hold the recording instrument, should be drilled straight and large enough to accommodate any recording instrument commonly used in conjunction with kymographic and polygraphic work. We have found that a ⅜-in. hole carries most of the instruments used in psychological and physiological research.

AN ADJUSTABLE MAZE FOR VISUAL AND NON-VISUAL LEARNING

By SHIRLEY MORRISON and T. W. COOK, Acadia University

The maze shown in top and edge view in Fig. 5 is an adaptation of the Shepard Adjustable Maze for visual as well as non-visual learning with human subjects. The top of the maze consists of 33 rows of wooden blocks separated laterally by ⅛ in. These blocks are 1 in. square on the top and ¾ in. deep, and each is raised from the base by a ½ x ½ x ⅜ in. block glued to the center of its lower surface.

A 1¾-in. no. 6 round-headed screw through the center of upper and lower block holds both in position on the base. Each lower block is also glued to the base. Be-

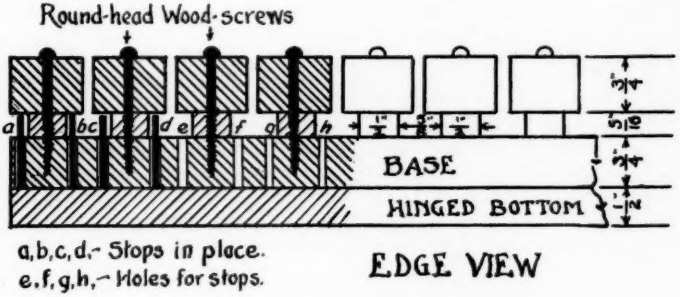

FIG. 5. ADJUSTABLE MAZE FOR VISUAL AND NON-VISUAL LEARNING

neath each of the four sides of every cube a ⅛-in. hole is bored in the base. As is shown in the edge view, each of these holes just clears the middle of one side of a supporting block. Stops can thus be inserted or removed by turning the maze over and raising boards hinged to the bottom.

The maze has the advantage of the Shephard type: it permits the construction of a large number of comparable patterns. In addition, since the stops are concealed and all paths seem open when viewed from above, the present maze can be used for visual as well as non-visual learning.

Our model is of hardwood and the cost (except in labor) is trifling. Brass would of course be preferable if expense is not a major consideration.

NOTES AND DISCUSSIONS

The Pronunciation of *Liminal*

In the galley proofs of the forthcoming *Dictionary of Psychology and Cognate Sciences* Professor H. C. Warren, the general editor, says that the word "liminal" should be pronounced "lĭm-." As the pronunciation recommended was strange to me, I queried it, only to have a rule of pronounciation quoted to me that "the long vowel in the noun is, in accordance with Latin etymology, shortened in the adjectival form."

The rule, if it is a rule, finds ready illustration (*e.g.* stamen-staminal; semen-seminal; nomen-nominal; crime-criminal; line-lineal; type-typical; bible-biblical; femur-femoral). But instances to the contrary—*i.e.* where the vowel in the noun remains long in the adjective—occur also with great frequency (*e.g.* cerumen-ceruminal; lumen-luminal; anus-anal; base-basal; locus-local; cement-cemental; Ceres-cereal; delusion-delusional; feud-feudal; fetus-fetal; flora-floral; lay-laical; labium-labial; legion-legionary; leo-leonine; lichen-lichenal; and the adjectival form of words ending in '-tion,' viz., notion-notional; sensation-sensational, etc.).

It must, furthermore, be pointed out that Latin etymology offers no sanction for the rule mentioned above. The long 'i' in 'līmen' and 'vīta,' for example, *remains long* in the adjectives 'līminālis' and 'vītālis.' But, as Jespersen says, "in Latin words it is not the classical quantity that is decisive, but the English school-pronunciation which followed its own rules."[1] The rule followed in this case is the so-called 'three-syllable rule' that "when a stressed syllable is followed by two (or more) weak ones, there is a strong tendency to shorten it."[2]

Rules, since they are known by their exceptions, and examples, since they are merely specific instances of usage, offer an insecure basis upon which to determine the correct pronunciation of any given term. The final arbiter in questions of this kind is authoritative usage. "Limen" and "liminal" are psychophysical terms. The psychologists, and in particular the psychophysicists, have, therefore, a right to be heard.

In order to discover the pronunciation actually used by competent men, I sent postal reply cards to 64 psychologists: to the past presidents of the American Psychological Association, to the psychophysicists of this country,[3] and to the heads of our foremost departments. I asked them to designate upon the reply cards, which carried the alternative pronunciations diacritically marked, the pronunciation that they used. Without having recourse to a 'follow-up,' I received 61 replies. Two of this number, however, count as blanks: one because my correspondent did not

[1] O. Jespersen, *A Modern English Grammar*, Pt. 1, 1909, 131.

[2] *Op. cit.*, 122. Cf. also the section on quantity in Romance (Latin and French) words (pp. 130-145), particularly pp. 139 f., where certain exceptions "in some modern (chiefly learned) words" are cited.

[3] I counted as psychophysicists all those who had published articles on that subject or were at present teaching courses in it.

know which, if either, was correct; and the other because both pronunciations were used indiscriminately. Of the 59 positive replies,[4] 32 indicated the usage of "lim-," and 27 of "li-."

An analysis of the replies, given in the accompanying tables, reveals striking differences among the three groups circularized. As there is considerable overlapping among the groups—many past presidents, for example, are heads of departments and some are psychophysicists—a dichotomy has been made for every group. Table I shows the usage of presidents and not-presidents. About two-thirds of the presidents (18 to 8) shortened the vowel in the adjectival form; whereas the majority of those not having held this office (19 to 14) used the long vowel.

TABLE I

SHOWING THE PRONUNCIATION USED BY PAST PRESIDENTS AND BY ALL OTHERS INTERROGATED

	"li-"	"lim-"
Past presidents	8	18
Not-presidents	19	14
Total	27	32

TABLE II

SHOWING THE PRONUNCIATION USED BY PSYCHOPHYSICISTS AND BY ALL OTHERS

	"li-"	"lim-"
Psychophysicists	21	6
Non-psychophysicists	6	26
Total	27	32

TABLE III

SHOWING THE PRONUNCIATION USED BY HEADS OF DEPARTMENTS AND BY ALL OTHERS

	"li-"	"lim-"
Heads of departments	13	16
Not-heads	14	16
Total	27	32

The results of the poll of the presidents stand strikingly in opposition to those obtained from the psychophysicists (Table II), for the great majority of the latter (21 to 6) use "li-." The non-psychophysicists use the alternative pronunciation, "lim-" in even greater proportion, (26 to 6). The division here is so marked that pronunciation may be taken as a rough indicator of interest in psychophysics. Indeed, one of my correspondents, a past president, a psychophysicist, and a head of a department, predicted this result. He wrote, " 'li-' is usual in psychophysics, 'lim-' more usual in psychical research and in the psychopathology of 1910 *et ante*."

The replies of heads of departments and not-heads are shown in Table III. Usage in both of these groups is about evenly divided (16 to 13, 16 to 14), but in favor

[4] One of the correspondents, a past president, wrote that he used "lim-" but preferred "li-." Since we are interested in usage, his reply was tallied as "lim-."

of "lim-." This division shows us, first, that eminence is about equally divided between the alternatives, and secondly, that eminence is as variable as the lack of it.

In order to discover whether pronunciation was geographically conditioned, a regional classification was made of the replies. As shown in Table IV, the proportion of "li-" to "lim-" is 8 to 6 in New England, 8 to 9 in the East, 2 to 4 in the South, 8 to 7 in the Mid-West, and 1 to 6 in the Far West. With the exception of the Far West, which shows a decided preference for "lim-," the returns are about equally divided. These results are not, however, fair indicators of geographical influence, for pronunciation in every case was doubtlessly established long before the correspondents attained their present positions. Since we are dealing with a technical term, one not learned in childhood but only in professional study, a better

TABLE IV

SHOWING THE PRONUNCIATION USED IN THE VARIOUS SECTIONS OF THE COUNTRY

		New Eng.	East	South	Mid-West	Far West
Present	"li-"	8	8	2	8	1
	"lim-"	6	9	4	7	6
A.B.	"li-"	7	10	1	3	6
	"lim-"	6	11	2	8	5
Ph.D.	"li-" (2)*	6	15	1	3	0
	"lim-" (4)*	8	10	3	7	0

* The numbers in parentheses indicate degrees from Germany.

TABLE V

SHOWING THE PRONUNCIATION USED BY THOSE OBTAINING THE DOCTORATE IN THE SEVERAL DECADES

	"li-"	"lim-"
Before 1901	3	12
1901–1910	5	12
1911–1920	11	7
Since 1921	8	1
Total	27	32

indicator would be the place of study. The replies were accordingly classified (1) upon the basis of the place of the first degree, and (2), since the term may not have been met sufficiently often in undergraduate study to condition pronunciation, upon the basis of the place of the doctorate. The results, given also in Table IV, show but slight differences. There is no indication of sectional preference.

Since the past presidents, the majority of whom received their professional training before the turn of the century, show a bias in favor of "lim-," it may well be that this pronunciation is the older, and that "li-," the pronunciation of the psychophysicists, has only gradually come into usage with the advance of psychophysics in this country. In order to test this conjecture, the replies were classified upon the basis of date of the doctorate (Table V). Of those receiving their professional training before 1901, the numbers using "li-" and "lim-" are respectively 3 and 12; between 1901-1910, 5 and 12; between 1911-1920, 11 and 8; and since 1921, 8 and 1. Though this analysis gives added warrant to our conjecture, it shows that there is no unanimity at any period and further that pronunciation is listing decidedly in the direction of "li-."

In view of these results a lexicographer is certainly not justified in being dogmatic. If he records usage he can do no less than to give both pronunciations of "liminal." If he wishes to standardize usage he cannot, without defying the present trend, declare for "lĭm-" and against "lī-."

Cornell University KARL M. DALLENBACH

ARE TONES SPATIAL?

The behavior of congenitally blind *O*s in experiments involving judgments of the extension or spatiality of those experiences which originate through hearing is important to the understanding of the nature of certain auditory phenomena. The question of the relation of tonal volume to visual space can be readily resolved if it can be shown that an *O* having no experience of space is yet able to make judgments of the phenomenal volume of tones.

In the course of an experiment on tonal volume the services of Miss S. V. Fladeland, a congenitally blind instructor at the Perkins Institute for the Blind, were enlisted for the purpose of determining what effect the absence of visual imagery would have upon an *O*'s ability to equate for volume two tones of different pitch by changing the intensity of one of the tones. This *O* was familiar with the concepts *pitch* and *intensity* but had never heard of *volume* as a phenomenal dimension of tones. Consequently it was necessary to train her to recognize volume. In order to do this, she was presented with a high tone (3000 cycles) and asked to notice its 'size.' Then she was given a low tone (200 cycles). She came quickly to understand what was meant by *volume,* and described the low tone as being "larger" than the high tone. The dependence of volume upon the intensity of the tones was made clear to her in an analogous manner, and she found that she could very easily make two tones equal in volume by changing the intensity of one.

The quantitative results obtained by the process of equating two tones in volume agreed closely with those obtained from normal *O*s.[1] After a series of 32 judgments it had become possible for this *O* to abstract the 'size' of the tones from their pitch and loudness without the aid of concrete imagery. Her report subsequent to the experiment was as follows.

"I used an arbitrary basis at first to establish my sense of volume. I compared the tone to a tube, or jet, through which steam passes to produce a sound, and by using my hand as a cup to fit the escaping steam I was able to determine its size. All of the tones used in the experiment would be close to the size of a not-too-expanded teacup. The high tones, however, were not only smaller, but they seemed to rise in space."

To the question, "Where were the tones?", she replied that they were on the right arm of the chair, in a position such that she could cup her hand around them. In order to cup her hand around the high tones, she had to raise her hand two or three inches above the arm of the chair. She explained that the raising of her hand for the high tones was probably due to the fact that in teaching speech to blind children she habitually made an upward gesture when she raised her voice.

The significance of the results obtained from this *O* is that, in the first place,

[1] An account of tonal volume as a quantitative function of frequency and intensity for various *O*s, including the present *O*, is accepted for later publication in this JOURNAL.

they show that volume is a true phenomenal dimension of tones in that it can readily be observed by an *O* unaided by visual imagery. Bannister,[2] in the course of his argument against the validity of volume as a category of experience, reported the case of an *O*, blind since the age of three, who "has never been able to experience or to understand the attribute of volume, although it has been suggested to him." It would be interesting to see whether, with systematic training, this *O* could not be taught to discriminate volume as Miss Fladeland did.

In the second place, the work with Miss Fladeland shows that the systematic identification of tonal volume with the truly spatial aspects of experience must be made with caution. Although the congenitally blind habitually employ the words *space, distance, size,* etc., it may well be that they understand by them something other than what the same words connote to a person with normal vision. Sir Wm. Hamilton[3] insisted that the observations of Dr. Platner[4] were sufficient proof that the congenitally blind have no concept or experience of space as we know it. The blind, he argued, experience only time and effort or motion, and the distance from one side of the room to the other means to them only so much time and so much movement. In 1844 Hagen put forth a similar argument.[5] In speaking of the congenitally blind he said, "Was uns Raum ist, ist bei ihnen bloss Zeit." More recently Goldstein and Gelb have shown that a patient who lacked all capacity for forming visual imagery was completely devoid of the sense of spatiality.[6] They concluded that through the *Tastsinn* alone one can never achieve *Raumvorstellungen,* and that consequently "es gibt eigentlich nur einen Gesichtraum." If this conclusion is true, it must follow that if a person who is incapable of experiencing space is nevertheless able to experience tonal volume, tonal volume is not a spatial experience.

It is a fact, however, that most normal *O*s report that their judgments of tonal volume are based upon the 'space filling" aspect of tones. They also report the use of visual imagery in most instances. As Goldstein and Gelb pointed out, a similar situation is encountered in the case of tactual localization. The argument of these authors was that the persistent presence of visual imagery as a concomitant of tactual experience leads the normal *O* to believe that touch, as such, has spatial qualities. If the same argument is applied to the discrimination of tonal volumes, we may conclude that it is only because of the presence of a visual surrogate that tones seem to have true phenomenal extent.

Some *O*s have directly denied the spatiality of the tones whose volume they were successful in judging. For instance, one of Rich's[7] *O*s reported: "It is a different kind of size from visual size. . . . Space is so visual to me that, when I open my eyes, it seems a sort of folly to me; but when I close my eyes, I feel the massiveness of the tones." In the course of an experiment conducted by the author, one of the *O*s said: "There is nothing spatial about volume. I match two tones; I

[2] H. Banister, Auditory theory; a criticism of Professor Boring's hypothesis, this JOURNAL, 38, 1927, 436-440.

[3] Sir Wm. Hamilton, *Lectures on Metaphysics,* II, 1861, 173-175.

[4] Ernst Platner, *Philosophische Aphorismen,* I, 1793, 446.

[5] F. W. Hagen, in R. Wagner's *Handwörterbuch der Physiologie,* II, 1844, 718.

[6] K. Goldstein and A. Gelb, Über den Einfluss des volstandigen Verlustes des optischen Vorstellungsvermögens auf des taktile Erkennen, *Zsch. f. Psychol.,* 83, 1920, 1-94.

[7] G. J. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.,* 1, 1916, 19 f.

don't compare two extents. It is more a matter of massiveness than size, because size implies space." This *O* also reported that he was aware of no visual imagery when he judged volume.

It would seem, then, that there is no good evidence that tones, as such, have truly spatial characteristics, and that one is justified in attributing space or extensity to auditory experience only if he is willing to accept the existence of a spatial surrogate as a sufficient basis.

Harvard University S. S. STEVENS

PSYCHOLOGICAL JOURNALS

The members of the senior author's class in the "Use of Psychological Literature" asked the question: "With a limited amount of money to spend what journals in psychology would be the better ones to buy?" In these days of reduced budgets this question should be widely pertinent. A corollary question might also be asked: "Of what psychological journals should even a small working collection contain a complete file?" The present note aims to suggest answers to these questions in terms of the actual use of these journals as represented in current American literature.

One number (usually the first or second) of the current (1933) volume of each of the following journals was searched for references to the literature: *Amer. J. Psychol., J. Abn. & Soc. Psychol., J. Appl. Psychol., J. Comp. Psychol., J. Educ. Psychol., J. Exper. Psychol., Ped. Sem. & J. Genet. Psychol., J. Gener. Psychol., J. Soc. Psychol., Psychol. Rev.* This list is a fair cross section of the experimental and theoretical literature of the present time.

In these numbers 1167 references to the literature were found distributed among 192 different periodicals, Table I gives the frequency distribution for those journals cited less than ten times.

Those journals cited more than ten times are presented in the following list with the numbers of citations in parentheses: *J. Educ. Psychol. (109), Amer. J. Psychol.* (104), *Psychol. Rev.* (76), *J. Exper. Psychol.* (70), *Ped. Sem. & J. Genet. Psychol.* (55), *Psychol. Monog.* (49), *J. Abn. & Soc. Psychol.* (46), *J. Comp. Psychol.* (41), *Arch. Psychol.* (31), *J. Appl. Psychol.* (28), *Psychol. Bull.* (28), *Amer. J. Physiol.* (21), *School & Soc.* (21), *Univ. Calif. Pub. Psychol.* (18), *Zsch. f. Psychol.* (17), *J. Gener. Psychol.* (17), *J. Anim. Beh.* (15), *Science* (15), *Genet. Psychol. Monog.* (15), *Brit. J. Psychol.* (14), *J. Physiol.* (13), *Pflüger's Arch.* (12), *Comp. Psychol. Monog.* (12), *Yearbk., N.S.S.E.* (12).

Thus we find that 12.5% of the journals provide 63.5% the references. Furthermore the 192 journals referred to represent only about 25% of the current publications that may be considered pertinent to psychology and its related fields. Louttit[1] has published a list of journals in psychology and its related fields which contains 1084 entries of which only 380 have ceased publication. Therefore we must conclude that, in so far as this sample is representative, American psychologists do not draw very widely from the available literature.

Table II presents, by half-decade intervals, the temporal distribution of these references. Nearly 75% of them were to papers published since 1920. Can it be that work published previous to this time is so thoroughly a part of the tradition that there is no need to refer to it?

[1] C. M. Louttit, *Handbook of Psychological Literature*, 1932.

Of the journals cited more than ten times, *i.e.* those listed above, there are only three which have not been referred to as early as the first half-decade of their life. Of these the *Univ. Calif. Pub. Psych.* started in 1910 but had a long period when no numbers appeared. *Science* started its new series in 1895 but it was not referred to previous to 1910. Eleven of the twelve references to the Yearbook of the N.S.S.E. are to the famous 27th number.

The third factor of interest is the frequency of references to the various languages. As shown in Table III, nearly 70% are to journals in English—the majority of which are American. Only 25% are to German journals although there are

TABLE I

JOURNALS CITED LESS THAN 10 TIMES

Citations	Journals
1	103
2	31
3	11
4	8
5	4
6	5
7	3
8	0
9	3

TABLE II

TEMPORAL DISTRIBUTION OF CITATIONS

Period	No.	%
1930–32*	266*	22.8*
1925–29	433	37.1
1920–24	171	14.7
1915–19	77	6.6
1910–14	90	7.7
1905–09	57	4.9
1900–04	24	2.1
1895–99	21	1.8
1890–94	11	.9
1885–89	4	.3
1880–84	6	.5
1875–79	2	.2
1870–74	2	.2
1865–69	1	.1
1860–64	1	.1
1830–34	1	.1

TABLE III

LANGUAGES OF JOURNALS CITED

Language	No.	%
English		
U.S.	111	57.8
Gr. Brit.	19	9.9
German	49	25.5
French	10	5.2
Italian	1	.5
Japanese	1	.5
Spanish (Argentine)	1	.5

* Only two years.

about as many journals published in that language as in English. Among the twenty-four most frequently cited journals there are only one British and three German.

Thus the answers to the original questions would appear to be that with a limited budget the twenty-four listed above should be purchased first. Another conclusion that might be drawn is that American psychologists make all too little use of the foreign literature.

Indiana University

C. M. LOUTTIT
LILLIAN L. LOCKRIDGE

THE TERM *Protensity*

The suggestion[1] that Professor Titchener coined the term *protensity* is incorrect. The first philosophical writer who used the term and its derivatives was Sir William Hamilton, who spoke of "Time, Protension or protensive quantity, called likewise

[1] Cf. E. G. Boring (*The Physical Dimensions of Consciousness*, 1933, 128), who refers to Titchener's first use of the term in a short note in this JOURNAL, 35, 1924, 156.

Duration." Quotations from Hamilton appear under these terms in the *New English Dictionary*.

Protensity appears also in James Ward's article "Psychology" in the *Encyclopedia Britannica*[2] in 1886. Here the term is said to be one of Hamilton's, and its meaning is also Hamilton's "time." In Ward's *Psychological Principles* (1918) the term is used, but it means not time in a physical sense but the temporal attribute of sensation. On p. 119 he writes: "The protensity ascribed to a sensation is—in a sense—the equivalent of the duration of the stimulus upon which its presentation primarily depends. But of this duration as immediately experienced, the subject, and not the stimulation as an external change, furnishes the measure, a measure that varies widely from subject to subject—according to the *tempo* of each—and even somewhat for the same subject from time to time—according to circumstances—independently of 'objective' duration."

The word also appears in the third edition (1913) of G. F. Stout's *Manual of Psychology* as a name for an attribute of sensation, but it does not appear in the 1899 edition of the *Manual* or in the *Analytic Psychology* of 1896.

Rice Institute FRANK A. PATTIE, JR.

A FURTHER NOTE ON THE 'WOLF CHILDREN' OF INDIA

Since the publication of the last note on the 'wolf children' of India,[1] I have received word from Rev. J. A. L. Singh, in whose orphanage the children lived.

Rev. Singh writes that the records of the two children are soon to be published, and encloses in his letter a statement regarding the children from Dr. S. P. Sarbadhicari, the physician who attended them from their capture to their deaths.

Dr. Sarbadhicari's account, which is quoted below in full, is of particular significance, not only because it gives us additional information, but also because of its corroborative evidence. His statement runs as follows.

"I was called upon for the treatment of Amala and Kamala, the wolf children, aged about 4 and 9 years respectively, on the 11th of September, 1921.

"They were suffering from emaciation, loss of appetite, etc. I suspected round worms, and on suitable treatment they passed out a very large number of them.

"As they could not be induced to take mixed diet their health could not be improved upon and Amala, the younger girl, died on the 21st of September, 1921, of Nephritis and general Oedema after suffering from the same for about 3 weeks. Curiously enough the older girl Kamala died also of the same disease on the 14th of November 1929. As a post-mortem examination was not made, the structural and pathological state of the kidney and other organs was not ascertained.

"I noticed the following peculiar characteristics in the elder girl, Kamala. She was shy, averse to remain in company, gave furtive looks of peculiar keen and penetrating nature, answered questions in monosyllables, and would not look up to one's face. She never showed any ferocious instincts in my presence.

"By the untiring zeal of the rescuer, the Rev. Singh, she acquired some sort of education, and could express herself tolerably well later on. There was great difficulty in feeding the girl with a mixed diet as she would hardly relish a vegetarian diet except milk.

"The acquired instinct of taking raw animal food by being reared by a

[2] *Encyc. Brit.*, 9th ed., 20, 1886, 65.

[1] For earlier notes, see P. C. Squires, 'Wolf children' of India, this JOURNAL, 38, 1927, 313-315; W. N. Kellogg, More about the 'Wolf children' of India, *ibid.*, 43, 1931, 508-509.

carnivorous animal was retained up to the last though all attempts were made to return to a normal human diet. The progress of education was very slow owing in part to the heredity of the subject (from aboriginal tribes) and also to nutritional defects, not contributing to the proper hormonal function.

"In my opinion, if the unfortunate wolf girls could have been kept alive and induced to take proper mixed diet with properly adjusted vitamins, improvement would have been more marked and they could have returned to an ordinary human condition from the stage of animal."

These children stand out unquestionably as the most striking examples of 'wild children' in our time. The publication of the complete records are awaited with interest.

Indiana University W. N. KELLOGG

THE FORTY-FIRST ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The forty-first annual meeting of the American Psychological Association was held at the University of Chicago, Chicago, Illinois on Thursday, Friday, Saturday, Monday, Tuesday, and Wednesday, September 7, 8, 9, 11, 12, 13, 1933. Registration figures reached an all time peak of 825 persons, classified as follows: 158 Members, 323 Associates, and 305 persons not affiliated with the Association.

At the business meeting, 3 Members were elected, 13 Associates were transferred to the status of Member, and 135 Associate Members were elected. It was voted to accept the invitation of Columbia University for the 1934 meeting and to extend the usual three-day meeting to four days. The forty-second annual meeting will be held on Wednesday, Thursday, Friday, and Saturday, September 5, 6, 7, and 8, 1934. Professor A. T. Poffenberger was appointed a member of the Executive Committee for 1933-1934, and William T. Heron, Christian A. Ruckmick, and the Secretary were elected as the Program Committee for 1934. Newly-elected officers are: President, Joseph Peterson; Treasurer, Leonard Carmichael; Directors, Harold E. Burtt, and Florence L. Goodenough; Division of Anthropology and Psychology, N.R.C., John F. Dashiell, Walter R. Miles, and L. L. Thurstone; S.S.R.C. Gardner Murphy.

The Association voted to continue affiliation with the Inter-Society Color Council for the year 1933-34 and appointed Forrest Lee Dimmick, Clarence H. Graham and Sidney M. Newhall as delegates to the Inter-Society Color Council. Reports prepared by the newly established Committee on the Ph.D. in Psychology (W. S. Hunter, Chairman, Edwin G. Boring, Harvey A. Carr, A. T. Poffenberger, and Lewis M. Terman) were given, showing present practices in regard to standards for the Ph.D. degree and analyzing the present status of supply and demand for trained psychologists. The supply of both Ph.D.'s and M.A.'s so greatly exceeds demand as to constitute a serious problem. The Association voted to continue the committee and instructed the committee to prepare the facts for presentation to prospective graduate students in psychology who are thinking of teaching psychology as a career, and to consider the problem of directing graduate training in psychology in such a way that professional psychological service can make increasing contributions to community life.

Twenty sessions for formal papers were scheduled and 132 papers were presented. Again it appears that the present day emphasis in psychology is genetic

as indicated by the volume of research activity devoted to the study of animals and young children. There is also marked interest in applied psychology, in the fields of abnormal and social psychology, and in the psychology of personality. Experimental work on sensation and perception and in psychophysics also exhibits marked vitality.

The policy of segregating research films and instructional films for special sessions seemed to meet with general approval. Nine research films and seven instructional films were shown. A Round Table on Instructional Films, with Walter R. Miles as chairman was held and was well attended. One of the twenty sessions for formal papers was devoted to papers on Clinical Psychology and two Round Tables on Clinical Psychology were also held. The Section on Clinical Psychology reëlected Edgar A. Doll as chairman for 1933-1934, and elected Harry A. Baker to be a member of the Executive Committee for 1933-1936. A total of 72 new Members and Associates was admitted to this section.

On Friday afternoon provision was made for the Psychological Corporation to hold a general meeting with papers and on Saturday afternoon a trip to the Mooseheart Laboratory for Child Research was arranged, at which eleven research papers were presented by members of the Child Research Staff.

On Monday evening, the Presidential Address, "The Vectors of Mind," was given by L. L. Thurstone. In brief, President Thurstone dwelt on the psychological significance of his recently developed mathematical method for determining the smallest number of factors which can account for the intercorrelations obtained from empirical measurements. Results of applying the Thurstone Method of Factor Analysis to measurements of vocational interests, to verbal and non-verbal intelligence test data, to attitude measures, and to data derived from case histories of psychopathic patients, were given to illustrate the usefulness and significance of this type of analytical approach.

Following the Presidential Address, the University entertained members in the Reynolds Club.

Professor Carr, his associates, and the administrative officers of the University of Chicago left no stone unturned to place the excellent facilities of the University at the disposal of our membership.

University of Minnesota DONALD G. PATERSON

Shepherd Ivory Franz: 1874-1933

Psychologists mourn the loss of one of the best loved and most productive of their number.[1] An early pupil of Cattell, and his assistant in developing the Columbia laboratory, Shepherd Ivory Franz, after obtaining his degree, devoted the next two years to physiology at the Harvard Medical School and taught this subject for a few years at Dartmouth before entering his characteristic life-work as psychologist in a mental hospital. This unique position he held first at McLean Hospital near Boston and then for many years at St. Elizabeth's in Washington. He was thus the pioneer, from the side of the psychologists, in a *rapprochement* between psychology and psychiatry, and his spirit of friendly coöperation combined with independence served him admirably in this relation.

[1] On October 14, 1933, at the age of fifty-nine.

He was the pioneer also in combining psychology with brain physiology. As a young physiologist who had come over from psychology he sought some way of making his psychology of use, and he devised a method of attack in which he combined the brain physiologist's method of extirpation with Thorndike's then new experiment in animal learning. Thus he had a plan for studying the cortical localization of the higher, learned forms of behavior. By the use of this method he soon obtained results indicating a lack of rigid localization. An act once learned by an animal and then lost through removal of part of the cortex could be relearned, and might be lost again on removal of another portion of cortex, and even learned a third time. Pursuing the line of inquiry thus suggested to him, Franz found from the autopsies of demented patients, as collocated with their case histories, that neither the location nor the extent of a cerebral atrophy bore any constant relationship to the kind or severity of the behavior losses. On mapping the monkey's motor area very minutely, he found individual differences in the precise location and extent of the excitable area for any given movement. From these and similar results he drew the general conclusion that the nerve paths and centers available for any given performance were by no means rigidly laid down once for all, and "that the paths of reactions are not the simple anatomical unities which have commonly been believed in but that these paths are diffuse and that anatomically as well as physiologically they are complex." Lashley, initiated by Franz into this line of thought and experimentation, has developed the evidence still further in the same direction.

As a corollary of his concept of a "labile cerebrum" Franz doubted the incurability of paralyses resulting from cerebral lesions, and he succeeded, by patient re-education combined with incentives such as games and the knowledge of actual improvement under practice, in restoring to many paralyzed patients a surprising amount of motility. He had some success also with aphasic patients, and with cripples of different types. He engaged in other varieties of clinical psychology, and one of his latest achievements was the redintegration of a case of multiple personality.

While serving as psychologist in St. Elizabeth's Hospital, he was also in charge of experimental psychology at George Washington University. The last decade of his life was spent as head of the department of psychology in the University of California at Los Angeles and in clinical practice. All in all, his career was one of notable service and achievement. He was for many years the editor first of the *Psychological Bulletin* and later of the *Psychological Monographs*. His fellow psychologists honored him in 1920 with the office of President of their Association. It is fortunate that we have an autobiography from his hands.[3]

Columbia University R. S. WOODWORTH

Otto Lipmann: 1880-1933

Otto Lipmann's birthplace was Breslau in Silesia. He was led by Ebbinghaus and myself into psychology, to which he devoted a quiet student's life until the day of his death. Lipmann never held a university position. When at last in February 1933 the University of Berlin offered him an appointment, it was too late, for the political situation prevented the beginning of his teaching activity. It also put an

[3] *A History of Psychology in Autobiography*, II, 1932, 89-113.

end to his editorial activity in which he had collaborated with me for twenty-seven years. A few weeks before the appearance of the last *Heft* of the *Zeitschrift für angewandte Psychologie* that bore our names on the title page, he succumbed to a heart attack.[1]

Lipmann's entrance into psychology occurred at the time when our science began to exhibit its great significance for application to different spheres of life. He seized upon applied psychology at its beginning, and his life work contributed to the realization of what was then only a program for the future. The term *applied psychology* appears in the titles of both of the enterprises that we began in 1906-07: the *Zeitschrift für angewandte Psychologie* and the Institut für angewandte Psychologie und psychologische Sammelforschung in Berlin (later in Neu-Babelsberg).

The Institute, a private undertaking of Lipmann's, was not a laboratory in the sense in which the term was then used: it made compilations, transmitted scientific information, established groups of workers for particular problems, and promoted methods for the tests and for statistics. Lipmann's principal collaborators at this Institute beside myself were Baade, Bobertag, Bogen, Kern, and Plaut. Of the great variety of investigations which issued from the Institute we need mention only the numerous *Aussage* experiments, the researches of Kern on arithmetical skill, and the work on practical intelligence directed jointly by Lipmann and Bogen. This latter work is recorded in the well known book, *Naive Physik*. The difficulty of the economic situation, however, forced Lipmann several years ago to give up the Institute, which had no support at all from public funds.

Otto Lipmann became widely known in scientific circles through his activity as the joint founder and the joint editor of the *Zeitschrift für angewandte Psychologie*. From the obituary which I have devoted to my dead friend in the last number of the *Zeitschrift*, I quote the following passage: "The literary judgment, habitually displayed by him and constantly developed by him through the years, has in a high degree contributed to the result that applied psychology has become articulate as an independent scientific field in the total movement of the science of our time. He also participated editorially in the publication of the other sections and the accessory publications of the *Zeitschrift*. Thus the forty-five volumes, the sixty-eight *Beihefte* and the forty-seven *Schriften* form a permanent monument to the indefatigable and prolific efficiency of Otto Lipmann."

Exceptionally broad and many-sided is Lipmann's series of scientific publications. The *Psychological Register* and the complete bibliography which Paul Plaut contributed to the commemorative number on his fiftieth birthday provide a picture of the wealth of achievement.[2] He wrote outlines of psychology for teachers and for lawyers, and he published for physicians a collection of psychodiagnostic methods. He enriched theoretical psychology with interesting hypotheses and ideas, of which we can mention here only the concept of *Periphertrieb* introduced by him. His predilection for questions of method led him in the earlier days to the working out of new experimental procedures concerned with the investigation of memory, *Aussage*, intelligence, types of perception, and so forth. In his later days it was

[1] This note has been translated, in the process of editing, from Professor Stern's German. Otto Lipmann was born on March 6, 1880. He died on October 7, 1933.

[2] P. Plaut, Die Arbeiten von Otto Lipmann, *Zsch. f. angew. Psychol.*, 36, 1930, 201-208.

especially the statistical methods that he zealously developed and improved. His book, *Abzählende Methoden,* is much used; and his last contribution to the *Zeitschrift für angewandte Psychologie* bore upon a statistical problem of osculation.

As a technical advisor Otto Lipmann was much in demand, since his readiness to help and his absolute objectivity won him the confidence of all who applied to him. Often, by an expert psychological opinion upon the depositions of adults and youths before a court, he was able to bring about a correct judgment so that an acquittal could follow. As a leader in the Berlin *Lehrerverein* he gave support for many years to the pedagogical-psychological group of workers.

In the last decade of his life Lipmann was drawn especially into economic inquiries, and in this field he gained a recognition for the value of psychological methods and points of view. He was scientific secretary of a commission of the Reichstag, which carried on an investigation of the problem of working hours, and the voluminous report of this inquiry is essentially his work. In the same way he coöperated constantly with the governmental board for economy. From these activities there developed in Lipmann the conviction that *Arbeitswissenschaft* should be established as a new independent division of science. His *Lehrbuch der Arbeitswissenschaft* of 1931 is a first and noteworthy attempt at a delimitation of this discipline.

Very close to Otto Lipmann's heart lay the international quality of scientific relationships, and his friendly contact with foreign scholars were many. He attended almost all the International Congresses of Psychology, and his American professional colleagues will still recall with pleasure his presence at the Congress in New Haven and the charm and graciousness of his manner.

It remains to be said that both external considerations and personal inclination kept Lipmann from close contact with the outside world and directed him to work in the quiet of his study, a fact which explains his peculiar mode of research. He formed his psychological insights, not so much from the direct study of human beings, as from the factual material that is furnished by experimental findings, statistical tables, school performance, industrial performance, economic data, and so forth. To arrange this unorganized material, to classify it and make it intelligible, to reveal the hidden facts, to transform it into psychological knowledge, that has been the principal method and the final mastercraft of Otto Lipmann. In this his own peculiar way he has made fundamental contributions to the progress of our science which will preserve his name in honored memory.

Late of Hamburg W. STERN

Eleanor Acheson McCulloch Gamble: 1868-1933

Eleanor Acheson McCulloch Gamble, Professor of Psychology and Director of the Psychological Laboratory at Wellesley College, died on August 30, 1933. Miss Gamble was born in Cincinnati, Ohio, March 2, 1868. She received her bachelor's degree from Wellesley in 1889 and her doctorate from Cornell University in 1898. In 1906-07 she studied and did research work with G. E. Müller at Göttingen.

Miss Gamble began her teaching at Western College for Women, Oxford, Ohio, in the field of Greek and philosophy and later was appointed to the staff of the Normal School at Plattsburg, N.Y. In 1898 she was appointed instructor in psy-

chology at Wellesley, in 1903 associate professor of psychology, in 1908 director of the psychological laboratory, and in 1910 professor of psychology. In 1930, at the death of Miss Calkins, she became head of the Department of Philosophy and Psychology. Miss Gamble was a member of the American Psychological Association, the American Philosophical Association, the Ninth International Congress of Psychology, Sigma Xi and Phi Beta Kappa. She was elected president of the Wellesley chapter of Phi Beta Kappa in the academic year of 1932-33.

While her career in psychology has left its impress largely on students in her elementary classes in psychology and while she was vitally and personally interested in them, both outside and in the classroom as is evidenced by the numerous influential collegiate committees on which she served, she has to her credit almost a score of articles in the professional psychological journals. Several of these were written in conjunction with Miss Calkins or with others. Five of the papers are summaries of the experimental and theoretical literature on taste and smell, a field in which she was a competent critic, but the majority of them are contributions of an experimental nature in the field of memory.

Miss Gamble also edited two volumes of the *Wellesley College Studies in Psychology* (1909, 1916) and was engaged with the editorial work on the third volume of studies at the time of her death. She was responsible for the direction of more than a dozen master's theses, the highest degree given in course at Wellesley. Most of these studies are experimental approaches to problems of memory, but there are a number on tactual and olfactory discrimination and at least two attack the problem of *Gestalt*. There are also almost fifty minor studies by undergraduates who worked in the laboratory under her direction. Here the fields of research are more varied and include problems in word-reaction, mental tests, attention, suggestion, motor dexterity and consciousness of self, in addition to a large preponderance of problems in the field of memory and association.

In August, 1933, her *Outline Studies in the Essentials of Psychology* were published. These consist of some thirty topics arranged in the order of her lecture program. The book is an attempt to integrate the existential psychology of Titchener with the self-psychology of Calkins. In the foreword she has paid her respects to the systematic presentations of McDougall and of Smith and Guthrie. Sprinklings of behaviorism and psychoanalysis are also admitted and the influence of G. E. Müller, William Healy, and Theodore Reik are acknowledged.

During last summer Miss Gamble finished her collaborative work on Warren's *Dictionary of Psychology and Cognate Sciences*. She coöperated in the subject-matter index covering volume 6 of this JOURNAL for the index of the first thirty volumes which appeared in 1926. Her experimental investigation of three variables in memorizing, which also summarized her own unique techniques of learning, appeared as a contribution to the *Washburn Commemorative Volume* of this JOURNAL in 1927.

Miss Gamble's publications and researches show that her main field of endeavor was the field of memory and association and that her methodology was the methodology of Müller and Pilzecker. But she was also keenly aware of problems in the olfactory sense-modality as is indicated by her competent critique of the Henning prism and by other writings. Sometimes these two major fields were combined, as in her elaborate monograph on memory which formed the first of the *Wellesley Studies*.

Olfactory, visual and verbal materials were used as the groundwork of the memorial series.

Although physically handicapped from birth by amblyopia in her left eye, and, since her first European trip, by a continually developing glaucoma in her right eye, Miss Gamble applied herself unfailingly to maintaining the experimental attitude at Wellesley. At the same time she was a dearly beloved and effective teacher. This was acknowledged by her election as honorary member of the class of 1926. During her last year of academic service she fulfilled a long-growing ambition of having the elementary course in psychology and philosophy put entirely on an elective basis. Due to her unique ability and popularity the enrollment, even in these days of economic pressure, did not decrease when this change was made. Her natural and disarming wittiness was an integral part of her philosophy of life, a good example of which occurs in the foreword to her published outlines. Systematically she was well-informed by constant contact with the literature, usually through the more efficient eyes of faithful readers. She also made a valiant and fairly plausible attempt to bridge the gap between the Titchenerian and Calkinsian points of view on the doctrine of self. It should be noted, too, that she contributed largely to the questionary form on imagery in Titchener's laboratory manual.

Miss Gamble constantly used her prominent synaesthesias in learning, especially toward the end of her life when in spite of several surgical operations her vision became increasingly poor. Her number-forms were also quite prominent. Since the early years when intelligence tests were new, she took an active interest in them and personally computed many of the correlations for use in the administrative offices. Her final examinations were remarkable instances of her ingenuity. They nearly always included some short story or fable upon which the formal questions were based. Her interest in philosophy and theological history continued to the end of her life but, of course, occupied merely the fringe of her immediate attention. Nevertheless, her course in logic also proved to be very popular with the students. A further indication of her open-mindedness in regard to new movements appears in the fact that she went to Vienna in 1923-24 in order to study psychoanalysis at its source.

Her death has fallen most heavily on Wellesley College but psychology also suffers. Among the immediate losses is a contemplated third volume of the *Wellesley Studies* which was to contain Miss Gamble's final synthesis of the studies on chance word-reactions and memory.

University of Iowa CHRISTIAN A. RUCKMICK

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Seven Psychologies. By EDNA HEIDBREDER. New York, Century Co., 1933. Pp. ix, 450.

Psychology seems at last, as this author notes, to be coming of age. Instead of systems and the controversies about them, we are beginning to get the descriptions and the histories of them. An insight into one's own faults and virtues is the mark of a seasoned maturity, and this reviewer ventures the opinion that, if most psychologists could read Professor Heidbreder's book and make her wisdom their own, psychology would already have passed beyond scientific adolescence. This is no ordinary book. The depth of insight, the accuracy of information and the facility of expression set it off from most of the psychological composition that we are all obliged to read. There is in the book one joke, tinged with irony: the title page calls the volume a "Student's Edition," a statement which can be literally true only if the word *student* means *scholar.*

Since there are systems and systems, Dr. Heidbreder set out to describe seven that are of especial importance to American psychologists. She begins with an excellent historical introduction dealing most competently with prescientific psychology and the beginnings of scientific psychology. These chapters are not a mere rehash of recent books, but show the author's own integration of the subject-matter and make a contribution to perspective in the history of modern psychology. Dr. Heidbreder knows her history of philosophy well enough to bring it in where it helps and to leave it out where it hinders. The seven psychologies are: (1) structuralism, meaning Titchener; (2) William James; (3) functionalism, meaning Dewey, Angell, and Carr (Carr at last gets his due); (4) behaviorism, meaning just Watson; (5) dynamic psychology, meaning Woodworth (who thus also comes belatedly into his systematic own); (6) Gestalt psychology, of the original German triumvirate; and (7) psychoanalysis, meaning Freud.

Except for behaviorism and psychoanalysis, it is approximately correct to say that the author disposes of these systems by getting enthusiastically expository about each in turn. She enters the stage from the wings of history as champion of Titchenerism, and then, presently, the scene shifts, and she is a keen and appreciative Jamesian; and thus on through other metamorphoses. However, she develops no schizophrenia in this process for she emerges at the end with an integrated totality. She has shown the reader just what each system is, just what good it did to psychology, just how it is related to previous thought, and why it is new and an advance upon previous thought. The reader can take his choice between the one and the many psychologies: he can see in psychology continuity and slow elaboration if he wishes, or he can see the stepped changes that new systems introduce.

The author makes a much stronger case for the unique and positive contribution of each system than any partisan could ever have made.

Watsonian behaviorism comes in for more definite strictures than any of the other systems. Dr. Heidbreder plainly thinks that Watson's escape from reality was much more thorough than Titchener's, because Titchener could see behavior when he would not entertain it, whereas Watson completely turned his back upon consciousness. Psychoanalysis, we gain the impression, is pretty good for what it pretends to be. It has merely the difficulty of being unscientific, of being an hypothesis formulated in such a way as to be forever unverifiable by scientific means, so that its value has to be judged by its appeal to belief and intuition. That is a devastating criticism to those who find it a devastating criticism, but hardly to the psychoanalysts.

A system comes into its own when persons outside its confines recognize its influence and begin to write about it. Self-baptism lacks virtue. Thus Dr. Heidbreder's greatest service is not her reëxposition of the *isms* of Titchener, James, and Gestalt, but her development of functionalism up to the present through Carr, and her clear formulation of the dynamic psychology of Woodworth. Each of these systems has been very important in psychology, neither has tried to 'ismise' itself, and it is appropriate and fortunate that recognition of their places in psychology should now come effectively from outside. Besides, it is more satisfactory so. Dr. Heidbreder can probably do better in respect of both generosity and clarity by Woodworth than he could do by himself. There is a great deal to be said for having the systematists give up trying to discover themselves and having the discoveries made by properly qualified experts.

The first chapter of the book, echoed by the last, deals with the function and operation of schools and of systematic controversy in scientific psychology. It answers perfectly the question that has long troubled the present reviewer, the question as to how scientific open-mindedness can be integrated with the amount of prejudice that is necessary to secure the drive for effective research. Does the truth always transcend the individual who has drive enough to be productive? Dr. Heidbreder says 'Yes' and 'No.' Science is in the hands of scientists and scientists are persons, whose wisdom is the unpredictable mixture of truth and error (influenced by modesty and arrogance) which all human wisdom is.

It seems probable that almost every candidate for the Ph.D. in psychology in America is going to read this book. They all have to know something about psychological systems, and for range, clarity, and lack of bias this book is without peer among the very few that exist. It thus becomes possible that the book may ultimately be found to have played an important rôle in the change of the psychological temper in America from youth to maturity. By extolling the value of controversy and the need for scholastic differences, Dr. Heidbreder goes far toward making both of them seem unimportant, for she lifts the controversalist who will attend to her up to a higher plane where he sees himself as a pawn in the great game of the evolution of a science. Your reviewer would not be alone in welcoming that state of affairs in which all the younger psychologists, besides being filled with the lust of battle, would also be prepared once in a while to ascend to the clouds and there to contemplate the meaning of the war.

It is a piece of good fortune for all these graduate students and for psychologists in general that Dr. Heidbreder can write English so happily. One might think

that academic psychologists would always write at least felicitously without the awkwardnesses that make meaning obscure or that distract attention from the thought. What else is an academic? However, the blunt fact is that Dr. Heidbreder stands out from the crowd. She is positively interesting. There is not an awkward sentence nor a stupid paragraph in the book. Her figures are vivid; her transitions are smooth; her choice of language is anything but trite. She has not the courage of William James to arrest attention by the judicious colloquialism, but give her time.

E. G. B.

The Laws of Human Nature: A General View of Gestalt Psychology. By RAYMOND HOLDER WHEELER. New York, D. Appleton & Co., 1932. Pp. xiv, 235.

The opening paragraph of the preface to this volume informs us that "the relativity movement in physics, the organismic movement in biology, and the *Gestalt* movement in psychology represent advances in a new conception of nature and a vast amount of startling experimental research which the new movement has promoted." On the next page, we are told, presumably of this volume, that "a new attempt has been made to direct the educated reader to the field of psychology as it appears when reinterpreted in terms of the new tool," the new tool being, presumably, the *Gestalt* theory. The reader might, therefore, expect to be told something about the "startling experimental research" and to have explained to him what *Gestalt* psychology is, as distinguished from any other sort of psychology, or at least what new advances it has effectuated. The eventual disappointment of the reader may of course be due to the fact that he is not 'educated.'

The first two chapters of the book are devoted to a historical account of the development of psychology from "primitive man" (who was a monist), through "the Greeks" (who seem to have been dualists), Aristotle (who failed completely to resolve dualism), William James (bracketed with Aristotle), "the Scholastics" (who "attempted to stem the tide of dualism by a return to the philosophy of Aristotle"), Descartes (who "put the finishing touches upon dualism"), occasionalism (apparently attributed to Descartes), Spinoza (whose psychophysical parallelism "is nothing but Descartes' out and out dualism over again"), "idealists" (not specified), "attention psychology" (whose exponents are apparently Descartes, Leibnitz, Herbart, and Wundt), to the "dead ends" of introspectionism (Titchener and Külpe), behaviorism (Watson), self psychology (Ward and Calkins), hedonic and hormic psychology (McDougall alone specified), and psychoanalytic theory.

This breath-taking journey through uncounted centuries, routed in less than seventy pages, is punctuated with conventional concepts of mechanism, vitalism, introspection, mind, matter and other familiars; and, for the most part, the bruised straw of these figures is left scattered along the path. Insight and instinct, however, are left still nicely tied together, and approved, which certainly is a tribute to some, at least, of "the Greeks."

This part of the volume is well worth reading, to impress the reader (educated or not), with the appalling need for more adequate cultivation of the rich field of the history of psychology; but it is to be hoped that it will not fall into the hands of students who have little orientation in this field. Space does not permit the reviewer to defend "the Greeks" against the manhandling to which they are sub-

jected, but Aristotle perhaps gets his revenge through the author's proclaiming as important truths (perhaps as interpretations in terms of the 'new tool') some of Aristotle's fundamental views. Early in the third chapter even, the author announces that "content is not the determiner of principle. Principle is the determiner of content."

In the remainder of the volume, the reader searches in vain for the "new tool" and its alleged revolutionary results. All the usual topics of a course in psychology are introduced in a somewhat helter-skelter fashion; many sweeping generalizations are formulated, some of them being commonplaces, others accepted maxims, and some are not entirely clear in their bearing. Intermixed with these are detailed statements of empirical fact, some of which are open to question; but the bearing of the details on the generalizations is seldom made out.

Among the statements which might be taken as bearing on the *Gestalt* theory we find some which boil down to the following. Forces do not produce results supernaturally, but in accordance with the environments in which they act. When objects which are in relations to other objects are treated in isolation, they are treated abstractly. Parts of wholes, in the wholes of which they are parts, are different from what they would be if not parts of the wholes. The cause of a phenomenon is not necessarily its immediate antecedent, nor even one abstractly isolated antecedent. Emphasis on accepted principles of this sort may be important for the instruction of raw students, but the psychologists can hardly be expected to become excited about them. Many statements seem, however to be mere definitional formulae, and for these the author does not stop to offer justification. For example: "From a functional point of view, energy, anywhere, may be construed as mind . . . mind as thus defined is not an idea until structured by thought." "Mind is determinism . . . is causation." Passing over the question how energy can be an *ism,* we wonder why the conventional usages of 'mind,' 'energy,' and 'causation' are not satisfactory.

Passing on, we find emphasis put upon the continuity of principles of explanation in the inorganic and organic worlds. (Page the Greeks! Several of them!) We find that the total response of the animal is the important thing. We find the 'experience theory' of the development of behavior denounced more than once, although apparently this theory is identified with the simple scheme of bonds. We find the author deeply puzzled over the development of visual coördination, singling this out for some reason from the vast magazine of psychological problems. We find the doctrine of the mental life condemned as a "hodge podge of inarticulate, unrelated elements." The integrative aspect of perception seems to be approved, and perception is defined as equivalent to cognition, with the remark that the outstanding characteristic of perception is meaning; although later, feeling is apparently classified as a meaning. Such formulations as "Parts derive their properties from wholes," "the whole conditions the activity of its parts," "parts emerge from wholes through a process of individuation," and "wholes evolve as wholes," may be abstract formulations of the integrative nature of perceptual process, or they may be intended by the author as more sweeping, and unsupported generalizations.

The exposition of the "law of configuration" in the fifth chapter, may be intended as an explanation, or basis for explanation, for the 'new tool.' If so, it

is far from enlightening. The "simple experiment of Wertheimer" in 1912, demonstrating apparent movement when there is no real movement somehow resulted in a "revolutionizing of modern psychology;" but why this phenomenon, familiar in our laboratories before 1912, failed to revolutionize anything until Wertheimer observed it, is not explained; nor are we told why it is more important than the various other discrepancies between 'appearance' and 'reality' which annoyed the epistemologists from Parmenides to Malebranche.

The chapter on the laws of learning presents a puzzle. First, instinct and the instincts are together drummed out of camp. "There are no instincts;" and, by implication at least, no instinct. Apparently, the author thinks the nervous system is incompetent to provide for variations in an instinctive response process. Yet we are shortly told that by a process of evolution there is produced in the animal "a nervous system dynamically so adjusted" that it conditions the complex and variable processes which we have been told cannot be instinctive. The discussion of learning which follows proclaims indiscriminately the failure and fallacy of all of the conventional conceptions regarding the learning process, and the "bankruptcy of associationism;" but no constructive substitutes are discernible. In one demonstration of the elasticity of the English language insight is set "*versus* determinism;" but insight seems to be merely a name for the fact that the determiners of response are patterns and not isolated stimuli, that in these patterns the relational nexus is highly important, and that, moreover, animals do learn, somehow or other.

It would, perhaps, be exaggeration to say that the volume is "a hodge podge of inarticulate, unrelated elements," yet it does create the impression of a note book in which vague ideas are jotted down from time to time for future elaboration or deletion, and eventually arranged in a formal order with a minimum of editing. What the real purpose of the book may be, except to serve as an outline of a course in general psychology in a metaphysical setting, it is impossible to decide. If the purpose expressed in the Preface is to be taken seriously, the reader has only three alternative conclusions from which to choose. Either the much heralded *Gestalt* psychology is a rather superficial bottling of an assortment of old wines in a new container, or else this volume is not in any wise an exposition of Gestalt; or else, finally, an interpretation in terms of a 'tool' can be grasped only by a peculiarly 'educated' reader.

Johns Hopkins University KNIGHT DUNLAP

The Use of the Self: Its Conscious Direction in Relation to Diagnosis, Functioning, and the Control of Reaction. By F. MATTHIAS ALEXANDER. Introduction by John Dewey. New York, E. P. Dutton & Co., 1932. Pp. xix, 143.

Those readers who know the author's earlier books—*Man's Supreme Inheritance* (1918), and *Constructive Conscious Control of the Individual* (1923)—are doubtless somewhat acquainted with the ideas and techniques outlined in this volume, the chief purpose of which is to present the history and development of that technique. Appeals have come to the author not only to make his methods available but also to provide for the training of other persons to extend and continue his work. He therefore announces in the present book the first payments to a trust fund for the endowment of an institution to train teachers for this work.

Mr. Alexander's central view is this: mal-functioning is not a matter involving only a single organ or individual parts of the organism; it is rather a bad use or direction of the entire self. Education in self-management is preferred as an idea to the popular notion of 'cures.' Conscious and reasoned control must replace 'instinctive' control and become fixed by actual use. The author was early interested in elocution and was successful enough to think of Shakespearean reciting as a career, but after some years his speaking was obstructed by throat trouble and hoarseness. No relief was permanently effected by the treatment given him by physicians. This led to various forms of self experimentation before mirrors to detect and avoid positions that were cramping to the larynx. Different hypotheses were set up and tested in a true scientific fashion. It was found that if the head were held upward and to the front the interference was avoided, but somehow the beneficial results in actual life-conditions when speaking was necessary were not realized—the trouble persisted. This resulted finally in the important discovery that the 'feeling' of the position taken was actually unreliable: when the author felt that he was successfully putting his head forward and upward (as if to enlongate himself) to relieve the pressure on the larynx, the mirror revealed to him that at the critical moment of attempting to speak he really fell into such obstructive bodily attitudes as actually to lower and bring backward the head. "By careful experimentation I discovered that I gave my directions for the new use in their sequence right up to the point when I tried to gain my end and speak, but that, at the critical moment when persistence in giving the new directions would have brought success, I reverted instead to the misdirection associated with my old wrong habitual use" (p. 30). This error, at first unavoidable, was attributed by the author to the lack initially of the sensory (kinaesthetic) data to indicate how to proceed in the performance of the right act, so that in the great hurry to get the act done he slipped into the old habitual attitude and 'feeling.' His solution was to learn to inhibit the act at this stage and to concern himself not with the act immediately to be done but with the means by which it could be directly carried out. He thus contrasts the 'end-gaining' with the 'means-whereby' motive or impulse. His system of training is to acquaint the student with his own wrong movements or adjustments and to help him get the "actual sensory experience" of the desired correct responses (but inhibit the critical act until he can maintain the set for these responses) and then to carry them out correctly. This is repeated until this new 'use of himself' gets to be habitual and is sensed as natural. It is, unfortunately, not made clear just how this new sensory experience is given the student, for we are told only "that the pupil receives from the hands of the teacher the actual sensory experience of the new use which he is consciously directing" (p. 104). Does he find valuable with persons this method of "putting one through the act" by physical means, which has proved of little value in training an animal, as American psychologists have found from experiments? Does he only guide the act gently when it is motivated and attempted by the student? Just what does he do? Those readers who are acquainted with such trial-and-error learning as star tracing, and ball tossing, will see much similarity between the author's method of bringing about control of new acts and those used by the learner, who has also to find out how it 'feels' to perform the new and correct act. But a trainer can do little for the subject directly in such experiments, except, of course, to tell him what to work for and use in the learning.

The author illustrates his methods and results fully in the training of a golfer, who could not keep his eyes on the ball just at the time of making the stroke, and in the education of a stutterer, who when excited fell into obstructive responses. The interest of physicians who have themselves had the training, or have had patients take it when their own methods failed to bring relief, is indicated in open letters in the appendix. The author claims wide fields of application for the method and indicates that it is not contradictory, but supplemental, to sound medical treatment; indeed, he maintains that diagnoses of many types of ills are not satisfactory if they do not include his method of functional diagnosis.

There are a few unfortunate uses of terms. 'Instinctive direction' in the wrong use of self (*i.e.* in wrong control) seems in several places to be used interchangeably with 'wrong habitual use.' There is, of course, a legitimate difference in these terms, and the latter indicates that something had interfered with the natural use and has developed a wrong use. The author might profit from an acquaintance with Jacobson's "progressive relaxation" technique and from some of Cannon's work, to neither of which there is any reference. His general point of view is unquestionably supported by experimental psychology of movement and of learning.

J. P.

The Mechanism of Nervous Action. By E. D. ADRIAN. Philadelphia, University of Pennsylvania Press, 1932. Pp. x, 103.

This book contains the second series of lectures of the Eldridge Reeves Johnson Foundation for Medical Physics. It is a résumé of the results of some of the experiments conducted by Adrian and his students at Cambridge, with a discussion of pertinent experiments of other investigators. No attempt is made to include all the literature of the topics treated. The first chapter gives a brief description of the development of electrophysiology and the changes in different types of nerves recorded by modern means of amplification. The changes are alike whether recorded from single motor or sensory nerve fibers or from total nerves and consist of a series of brief potential changes or impulses. In a given fiber these impulses are of uniform size but vary in frequency, that is, gradation of activity is accomplished in terms of the frequency and not the intensity of the impulse. The amplitude of the potential changes varies in nerves of different size. Adrian accepts the membrane hypothesis as the best explanation of the conduction of these potential changes down the nerve fiber. The next chapter is a discussion of the action of those sense organs which are stimulated mechanically, *e.g.* the muscle spindles, the Pacinian corpuscles and the hair endings. These endings differ in their rate of adaptation to a continuous stimulus showing differences in the time course of their discharge. Some react with a persistent discharge as long as the deformation lasts, while others respond only during the actual movement of deformation. In all these nerves "the frequency of the discharge depends on the rate of increase, the final intensity and the duration of the mechanical change." The rhythmical nature of the discharge to a continuous stimulus is explained in terms of a persistent depolarization of variable extent on the surface of the sense organ. The difference between pressure and touch sensations is explained in terms of the rate of adaptation of the sense organ, pressure endings adapting slowly and touch endings rapidly. The adaptation of the auditory nerve is rapid, as is touch, and in either sense the intensity of the stimulus is judged on the

basis of the number of impulses making up each volley. He discusses the work of Wever and Bray and suggests that "the high frequencies found in the composite potential waves are most probably due to many fibers, each responding at a submultiple of the stimulus frequency and arranged so that each wave of the stimulus finds some fibers ready to respond to it." The pitch of a note is then judged from the frequency of the volleys without regard to the frequency in each nerve fiber.

In his discussion of pain (Chapter III), Adrian has revised some of his earlier views to conform to more recent findings. The ending responding to light touch does not give rise to pain when more intensely stimulated as was formerly held. The unmedullated fibers noted by Ransom and the small, slow conducting group C fibers of Erlanger and Gasser are thought to be concerned with pain although this sensation is not to be restricted to these fibers or to any particular group of pain fibers. As the impulses in the fibers giving rise to pain do not vary in size the intensity of the sensation is explained in terms of the summation of the changes resulting from the several impulses. The ultimate quality of sensation cannot be wholly attributed to specific fiber reactions. Factors of chronaxy, changes in the discharge frequency, and differences in fiber connections are other variables to be considered.

Chapter IV considers impulses in motor nerve-fibers. The translation from nervous change into movement depends on a fusion of the effects of repeated impulses. Gradation in contraction results from changes in the frequency of the impulses in individual nerve fibers and in the number of fibers which are stimulated to activity. Adrian suggests that much confusion would be avoided if the "all or none principle," through rewording, were limited to an "all-or-nothing relation between the stimulus and the propagated disturbance" and not used to cover all the local effects produced in a nerve by stimulation. Persistent discharge in sympathetic nerves do not show the regularity found in sensory and motor nerves, probably because there is "no opportunity for a steady excitation."

The last chapter deals with the activity of nerve cells and reviews some of the more recent experiments at Cambridge on the potential changes in the brain stem. Although admitting that the reflex arc has been a valuable conception in nerve physiology, Adrian opposes the extensive use that has been made of it and cites a number of experiments which tend to show the presence in the brain stem of automatic discharges not dependent upon afferent control. Rhythmic slow breathing after vagotomy is explained as due to an automatic beat of the respiratory center. The hypothesis is advanced that slow potential changes taking place in the nerve cells and dendrites give rise to these rhythmic discharges. This depolarization of the cell is not the explosive type found in the nerve fiber but takes place at a slower rate, persists over longer periods and is capable of gradation. This slower surface breakdown gives rise to a discharge of increasing and decreasing frequency. The periodic discharges in injured nerve fibers are ascribed to a similar mechanism, the synchronization of these discharges resulting from the electric fields set up by the impulses. The author presents these views only as tentative hypothesis, frankly admitting that definite conclusions about functional mechanisms of the central nervous system are still premature.

The book is very readable. It is adaptable for courses in physiological psychology and should be readily understood by students having had the usual background of experimental work. A bibliography of 62 references is appended.

University of California. C. W. BROWN

Present Day Psychology: An Objective Study in Educational Psychology. By ALENE RALSTON and CATHERINE J. GAGE. Philadelphia, J. B. Lippincott Co., 1931. Pp. lxiv, 404.

This book attempts to meet the three-fold purpose of putting "the fundamental facts of educational psychology in a language simple enough for the uninitiated," "to answer the many demands from students for something easy and good, and at the same time authentic, for their parents and friends who are not familiar with psychology;" and "to help the teacher in elementary school service who entered the profession at a time when the facts of modern educational psychology were not available" (p. vii). The authors attempt to maintain an objective point of view and make no claim to originality.

Psychology is defined as "an attempt to understand life as it is lived;" but a departure from objective methods comes early when the reader is told that guidance leads to controlled behavior, and that "control presupposes an urge, *a purpose, a reason*" [italics reviewer's]. As to the fields of psychology, adolescence is characterized chiefly according to the old view that "natural tendencies" ripen and create "storm and stress" problems. Such types of individuals as visiles, audiles, motiles are mentioned conspicuously in this *Present Day Psychology* as individual psychology problems; and social psychology is made to deal "with the reactions of crowds and mobs as contrasted with those of individuals" (p. 6).

The general outline of the book follows closely Thorndike's three volumes, *Educational Psychology,* with fillings-in from other authors and from the authors' own interpretations. "Satisfaction and annoyance" are not regarded merely as aspects of response, but as *verae causae* actually determining the sequence of responses (p. 10). The recent controversy about instincts, which for a time took so much space in the journals, has not in the least affected the authors' fidelity to Thorndike's view published in his *Original Nature of Man,* approximately twenty years ago. The excellent work of Cannon on the emotions, extending through many years and widely known, is not even mentioned in the text. This is true also of Lashley's work on learning and cerebral functions. Certainly the importance of these omissions and of others relating to recent contributions on the selective factors in learning, including Thorndike's repudiation of his earlier position on frequency and recency as selective factors, serve to indicate that the book is hardly a "present day" psychology, even in its own field. Similar omissions of recent work in association and memory are noticeable. But psychoanalysis is not overlooked; neither is it handled uncritically. While it is pointed out that psychiatrists (*sic!*) and psychologists "were slow to take up psychoanalysis," G. Stanley Hall and R. S. Woodworth are named as psychologists "who became interested in psychoanalysis" (p. 240). This subject and behaviorism, dynamic psychology, purposivism, and Gestalt psychology, all receive some consideration, but, as we have seen, much excellent experimental work is omitted. Functionalism and structuralism—treated among other current trends in psychology—are defined and discussed briefly, and we are informed that William James and Titchener "were advocates of" these "opposed views" (p. 246). Thus the authors frequently reveal a naïveté and an unfortunate lack of contact with developments in psychology. There are tendencies, also, to reflect the popular fear of mechanism despite an ostensible acceptance of the methods of objective science. 'Freedom' seems to be thought of in the old absolutistic manner rather than as a relative independence of control by immediate circumstances.

There is serious confusion in the treatment of color mixture of pigments (by the subtraction method) and mixture with color disks on a color wheel (mixture by addition and in part also by subtraction). Also Hering's theory does not assume, as is stated, "three pairs of colors." To say that "in imagery no stimulus is present" is grossly erroneous. Moreover, the definition of imageless thought is inaccurate and misleading, and it is certainly a reflection on the careful experimental studies bearing on this problem to say that "one may test himself by a little introspection" (p. 252). Moreover, the old notion of the "composite image" was certainly quite different from that of imageless thought. The statement that the after-image "is due to retinal fatigue" is subject to challenge, as is also the one (p. 254) that "no mental state is possible without the first pure sensation."

Several inaccuracies occur in the treatment of the nervous system. Here, under the caption, "The Autonomic Nervous System" is the following interesting information: "The cranial or upper part allows eating; the sympathetic or middle part permits fear or anger; the sacral or lower part reënforces love. There is an antagonism of the first for the second and third parts. The cranial system favors digestion. This is not true of the other two. There, emotional excitement is paramount. Hence, the wisdom of no food, or little, when one is grieved or worried or in love. . . . The sight of, hearing or thinking about, food, anger, or love will arouse one of the three emotions" (65 f.). A student in love, for instance, may be somewhat puzzled as to what to do at meal time if he has absorbed this half-knowledge!

Association fibers are misconceived. Memory is curiously defined as *"the awareness in consciousness* of an event or fact that has been experienced before" [italics reviewer's]. Old views of attentions are given and we read that "non-voluntary attention is the kind one gives because he wants to attend." Non-psychological, and formal treatments are also given to thinking, concept, reasoning, and will. Several statements are positively wrong; *e.g.* "The function of the will is to cause or to inhibit action" (p. 262).

The last third of the book deals in a practical way with tests and statistical terms and procedures. Concrete illustrations abound but do not obscure the principles involved. There is a wrong interpretation of correlation coefficients (p. 369), and unfortunate errors in the forms of the normal distribution curves shown (pp. 372 and 374).

The book seems to have grown out from needs felt in elementary teacher-training institutions, and for the purposes stated—except for being an *authentic* text—it has certain commendable features. Numerous references given in footnotes are reproduced in the nine-page classified Bibliography at the end of the book. Exercises and questions follow each chapter. It would be fortunate if the book could have such thorough revision, and be put so fully abreast with present day psychology, as to receive the recognition of psychologists. It is desirable that authoritative and excellent texts arise in the fields where they are needed.

J. P.

The Delinquent Child. By FREDERICK P. CABOT and others. The White House Conference on Child Health and Protection. Report of the Committee on the Socially Handicapped. New York, The Century Co., 1932. Pp. xx, 499.

This is without doubt the best general book on the delinquent child. The work includes seven chapters: "The Delinquent Child in Relation to Himself;" "The

Child in Relation to His Family;" "The Child and the School;" "The Child and the Church;" "The Child and Industry;" "The Child and Community Influences;" and "The Child, the State, and the Municipality." A "General Report" of 31 pages ably summarizes the viewpoint and recommendations of the Committee. An Appendix of 90 pages provides much factual material, some of it new, on which the authors base their beliefs. Extensive "Subject-Reading Lists" are also provided, as well as a generous bibliography of 384 titles. A ten-page index completes the book.

This White House volume bears much evidence of careful, conscientious, and competent effort on the part of the Committee. The Committee has expressed current beliefs so clearly and well that one wonders if this book will not, by the eminence of its authorship, serve to indoctrinate as well as to illuminate. Such an effect would be most unfortunate; for there is very much in this volume which demands deep critical thought.

We find in the book the usual emphasis on the individual delinquent *vs.* the delinquency; and the usual emphasis on the essential normality of the behavior-mechanisms that move the child to delinquency. We find also the usual arraignment of society's failure to satisfy the essential needs of its children. The Committee, however, fails to advance much beyond these clichés. To the writer, it seems that progress in the problem of delinquency is more likely to be made on a very different tack from that implied by the Committee. In the first place, we must cease paying our respects to "the naturalness and ubiquity of those processes which lead up to" delinquency. Delinquency is neither ubiquitous nor natural among the greater proportion of the population. It is well enough to adopt a sympathetic, humanitarian attitude toward the delinquent; it is quite another thing to let this attitude warp our insight into delinquency. The prime requirement, now, is to discover and stress the ugly *differences* (both qualitative and quantitative) between the delinquent and the non-delinquent child. We shall not make the progress that we should, until studies of delinquency are broadened to include (more definitely than they are included in the present volume) companion-studies of the non-delinquent.

In the second place, we think that if society is to be arraigned, the Committee should have done a more thorough and honest job of it. Possibly the White House affiliation embarrassed the Committee in this regard; possibly the personnel of the Committeee—which is lacking in a fair representation from sociology and anthropology—was inadequate for the performance of this unpleasant task. Whatever the cause, the fact remains that issues of the utmost importance receive little or no recognition in this volume. Given a prevailing individualistic and skeptical philosophy, operating in an increasingly urbanized civilization, with little or no eugenic restrictions, can the proportion of abnormal self-assertion (both in the direction of delinquency and neuroticism) ever be significantly reduced? Given a money-economy, the profit-system, and an extremely uneven distribution of wealth, advantages, and opportunities, shall we ever be significantly relieved of those who seize society by its forelock in an effort to get illegally what they cannot in fact get by any other means?

One further issue must be mentioned; this is the Committee's slighting of the influence of heredity as a cause of delinquency. No one nowadays would urge that crime is directly inherited as such. Is it not significant, however, that feebleminded-ness—certainly largely hereditary—is a definite handicap, both to society and the

child, in the problem of delinquency (p. 65)? Who can say to what extent other hereditary factors operate in any individual case? The Committee strenuously urges that we study the individual delinquent; but on closer inspection it seems that this study is to be restricted mainly to discovering the *environmental* causes of the delinquency. Lange's famous study of criminality in twins suggests that we might better study the factors of heredity. From the point of view of practical therapeutics, of course, environment is the proper center of attention. But a scientific analysis would certainly go beyond the merely practical and therapeutic phases of the problem. We think that the predominance of the clinical, therapeutic viewpoint has injured the scientific value of the present book. The predominance of the clinical viewpoint probably accounts, too, for the occasional crusading note in certain passages of the volume.

The book suffers from excessive generalization, and insufficient recognition of the need for more strictly quantitative expression. For example, the statement that "adult crime has its roots in juvenile delinquency" (p. 19) is probably truer of the crude crimes of violence than of bribery, embezzlement, forgery, dishonest bankruptcy, stock-swindling, and crooked business practice in general. A strict quantitative statement of the influence of early delinquency on later crime is not yet possible; but this very absence of quantitative data should have led to greater caution and qualification.

Of the individual chapters, "The Child and the School" seems the best written. "The Child and the Church" makes little contribution.

Despite our criticisms, we still consider, as stated at the very outset, that this is the best general book on the subject of child delinquency. The book is a landmark, and it should be owned by every single worker whose interests touch on the broad field of delinquency.

Institute of Child Welfare
University of California

HERBERT S. CONRAD

History of Psychology in Autobiography. Vol. I. By JAMES MARK BALDWIN and others. Edited by Carl Murchison. Worcester, Mass., Clark University Press, 1930. Pp. xvii, 516.

We have here the first volume of a series designed to give the history of psychology in autobiography. The idea of such a series originated with Boring in 1927 while preparing an article on Titchener (cf. this JOURNAL, 38, 1929, 489-506). Realizing that many important facts concerning an individual's scientific development could be obtained only at first hand, he proposed that such information be placed on record in this JOURNAL in a series of autobiographical articles by the foremost psychologists of the world. Attractive and worthy as this suggestion appeared, it could not be adopted without radically altering the scope of the JOURNAL. The idea was, then, temporarily abandoned only to be presented later to Dr. Murchison of the Clark University Press, who, fortunately, found it both attractive and feasible; and we now have as a consequence the autobiographies of 15 eminent contributors to psychology.

The authors of these autobiographies, and of those subsequently to be published, were selected by an international editorial board. Eminence and seniority in psychology were not the sole bases of inclusion, as individuals in peripheral and neighboring fields have also been invited to contribute.

This volume contains the autobiographies of James Mark Baldwin, Mary Whiton Calkins, Edouard Claparède, Raymond Dodge, Pierre Janet, Joseph Jastrow, F. Kiesow, William McDougall, Carl Emil Seashore, C. Spearman, William Stern, Carl Stumpf, Howard C. Warren, Theodore Ziehen, and H. Zwaardemaker. The death of Calkins and Zwaardemaker, since the publication of the volume, lends point to the timeliness and need of the undertaking.

The autobiographies are very uneven in their value to psychology, and show the need of a firm editorial hand. Dictation on the part of the editor is of course out of the question with such a galaxy of luminaries, and requests for revision might have led to withdrawals, but the kind of article desired should have been stipulated in advance, and non-compliance should have met, despite the risk of withdrawal, with diplomatic requests of revision.

Some of the autobiographies defeat the purpose of the series, as they tell us little or nothing about the author's intellectual and scientific development. One is an out and out success story; one is a mere résumé and review of the author's books and papers; and at least two must be described as "reflections at sixty." Some, on the other hand, are revealing and fulfill the purpose of the series. McDougall's article, for example, contains the key to his psychology. He writes with a frankness and disinterestedness that few could bring themselves to emulate, and no one can read his self-analysis without obtaining a fuller understanding and appreciation of his psychology.

Pertinent biographical data, such as one would expect to find in an autobiography, are frequently omitted. For example: 9 of the 15 authors do not give place or date of birth; 4 do not give their full names, and 3 do not give even their first names. If this information be desired, it must be sought elsewhere!

Six of the articles are translations: Claparède, Janet, Kiesow, and Ziehen from original manuscripts, Stern and Stumpf from articles published in *Philosophie der Gegenwart in Selbstdarstellungen* (vol. 6, 1927, ed. Dr. Raymond Schmidt). The translations, with the exception of Janet's article, are good.

The volume has not been carefully proof-read. Numerous omissions and inversions of letters occur, and in one instance (p. 155, center) the linotype slug is inverted.

K. M. D.

Endocrine Medicine. By WILLIAM ENGELBACH. Springfield, Ill., and Baltimore, Md., Charles C. Thomas, 1932. 3 vols. Pp. 1800, 933 illustrations.

The matter of internal secretions is more and more claiming the attention of the medical and allied professions because of the profound influence which these secretions exert on growth and metabolism and on the specific capacities of other tissues of the body.

In the past decade a number of volumes have appeared on the subject but the march of knowledge in this new field is so rapid that a timely summary is needed at frequent intervals.

Endocrine Medicine by the late Dr. William Engelbach is probably the foremost compendium in this country on the subject of glands of internal secretion up to 1932. The three volumes are well printed and bound. They contain a wealth of well selected illustrations, tables, charts, and clinical data from reliable sources. There is an appreciative foreword by Dr. Lewellys F. Barker. The arrangement is systematic, the treatment is orderly and the text is commendable for its clarity

and avoidance of obscurity. The volumes deal with the hypophysis, thyroid, parathyroid, gonads, thymus and suprarenal cortex, and the disorders resulting from their secretory disfunctions. They do not include the suprarenal medulla, the pancreatic islands, and liver: the triad of organs which deals primarily with the functions of carbohydrate metabolism.

Volume I, entitled "General Considerations," is tersely written and abundantly informative on the anatomy, physiology and nomenclature of the glands, and their better known secretions. It discusses the functional units, their interrelations and their effects on the non-endocrine systems. The causes of glandular disfunction and the most available diagnostic procedures for the recognition of such disturbances are fully presented. Emphasis is placed on a thorough medical survey in all cases. Numerous tables and charts of anthropometric and physiological measurements, of tests and other data are provided, by reference to which the presence of normal function or disfunction may be judged. In conclusion the author aims to assay the relation of endocrine disorders to public health. Here the student of abnormal psychology may derive some idea of the relation of endocrine disfunction to abnormal mental development. The author estimates that about 10% applying for medical opinion in whatever field are afflicted with some uncomplicated endocrine disorder.

Volume II deals with simple and combined disorders of the thyroid, hypophysis, gonads, and thymus as they affect children in infancy and in the juvenile period. It may be of special value to the child psychologist interested in the interpretation of certain rather common aberrant physical and mental types. The common birth injuries and mental deficiencies are not included in the treatise. Anthropometric and roentgenologic methods and physical comparisons are extensively used for differential diagnosis. In this volume many examples typical of endocrine disorders of childhood are illustrated. In the young the physical and constitutional characteristics are usually well defined as the unbalanced growth processes proceed to maturity. In the concluding chapter only a brief reference is made to the relation of such conditions to mental activity and intelligence.

Volume III deals in a similar way with adolescent and adult endocrinopathies. The treatise is largely clinical, based on reference to experimental data. It is well illustrated by typical cases.

It is a growing concept that the secretions of this group of glands are so essential for the internal chemistry and growth processes of the body that they ultimately shape the physical and functional constitution of the individual, and in the broad sense that of the race. Because of this relation there is much in these volumes that will be of interest to students of genetics, anthropology, physiology, and abnormal psychology, although these branches have been invoked only as handmaids to the main purposes of clinical endocrinology.

Cornell University Medical School JAMES W. PAPEZ

Sorcerers of Dobu: The Social Anthropology of the Dobu Islanders of the Western Pacific. By R. F. FORTUNE. Introduction by B. Malinowski. New York, E. P. Dutton & Co., Inc., 1932. Pp. xxviii, 306, 7 appen., 8 pl., 5 fig., drawing, map.

The literary as well as the scientific world is indebted to the author for the wealth of primitive legend, dance song, and "secret" ritualistic spells contained in *Sorcerers of Dobu.* In addition, through a series of concrete pictures the reader

is enabled to view the primitive mind at work and to see the functioning of this lore in the lives of the Dobuans. The text is replete with factual and interpretative data, resulting from a five-months' study of exceptional method and success in the "unmissionized island of Tewara."

Adoption into a tribe, with immediate use of its language ("acquired by contagion"); allaying the suspicion and jealousy accorded the stranger; procuring a house and garden, made with due magic ritual; joining in land and sea expedition for hunting or barter; sharing in dour looks for truculent trader or shout of triumph at bringing home the wild boar (substitute for former cannibal victim); trussing up or drenching with cold water the maniac "running amuck with a spear;" healing with salts and good food the victim "dying" from sorcerer's spell—these were the intrepid means of first hand observation. Moreover, the author listened to the pronouncements of government official, the "innocent" interpretations of near-by missionary, the island gossip of trader living with native woman, and the whispered revelations wrested (by white or black magic!) from the most secretive of the sorcerers themselves.

Magic, with the fears, hopes, and cruelties it engenders, is at the basis of every act of Dobuan life. Sorcery (if practised by a male) or witchcraft (if by a female) controls or accounts for all the toward or untoward events, from change of season to a birth or a death. The diviner must determine the secret enemy who wished, ergo caused, demise. Though naturalistic means may be employed in the growing of yams or the poisoning of an enemy, yet it is the letter-perfect weaving of a charm that controls the mystery beyond man's reach—the alchemy of growth or decay. Social obligations, moral and economic, are observed, if at all, through fear of a vengeful magic. This primeval fear, not the white man's government or religion, is as yet, according to the author, at the root of all personal control.

Sex life among the Dobuans is marked by the speech prudery and actual depravity attributed by twentieth century sensationalists to nineteenth century Nordics. The *"susu,"* or matrilineal clan, is more powerful than the marriage group. Exogamy is practised, always with the hazard of entering an enemy group whose magical powers are unknown. Divorce is common, where the debacle is not ended by attempted or threatened suicide.

The author questions the ability of the church by milder teachings or of the government by severity of punishment to stamp out sorcery. Recognition of sorcery, by punishment for instance, but confirms the natives' belief in its reality, prevents its dying from its own failures, and serves to keep intact a field for the anthropologist. He questions also the oft repeated stories of "the ease with which the native dies" from sorcery alone, unassisted by poison administered by an enemy or induced by suspended vital functioning. Whether or not the hypnotic spell cast by "meeting the enemy face to face" is profound enough to result in death; whether or not in Dobuan or other societies strong matrilineal loyalties may be a factor in clannishness or exclusiveness; and whether or not there may be some humane shortcut from primitive, barbaric fears and cruelties to a utopian fellowship and coöperation, are questions intriguing the interest of the reader and inviting the further investigation of the physiological psychologist and philanthropist as well as the social anthropologist.

Woman's College of Alabama SARAH M. RITTER

Psychology and Psychiatry in Pediatrics: The Problem. BRONSON CROTHERS and others. The White House Conference on Child Health and Protection. Report of the Subcommittee on Psychology and Psychiatry. New York, The Century Co., 1932. Pp. xiii, 146.

The deliberations and investigations of the Committee, here reported, deal largely with the question of guidance of normal personality development and the treatment of personality difficulties in children. Is this a task for the general medical practitioner, pediatrician, psychologist, psychiatrist, clinical psychologist, social psychiatric worker, parent, educator, or for a combination of two or more of these? The obvious answer, of course, is that responsibility lies with all of those mentioned, depending on particular circumstances and the nature of any disorder present. "Without question, the home and the school must do most of the work." The problem, therefore, resolves itself into one of leadership and initiative. The Committee here makes a clear-cut distinction between guidance and preventive measures, on the one hand, and treatment of palpable aberrations of the individual personality on the other. The former aspect of the problem is left largely to the parent, teacher, and psychologist. Individual distress, however, is held to be primarily the concern of medical men. "Adequate medical care of the child cannot be given without intelligent attention to any intellectual and emotional difficulties which may be present. . . . When trouble arises and the individual child is in distress a well informed and alert physician is the obvious adviser."

Since the problem is, after all, a practical one, it would be beside the point to take issue with the questionable justification of even a working dichotomy between normal and abnormal, prevention and therapy. This schism, of course, is common in medical thinking and practice. What can and should be seriously considered, however, is the practicability and wisdom of placing primary responsibility for the mental health of his patients on the already heavily burdened shoulders of the family doctor. There are, clearly, arguments both for and against such procedure.

Quite certainly the prestige and confidence enjoyed by physicians gives them an important advantage in any kind of therapeutic work. Certainly, also, the capable physician has learned to supplement his medical procedures invaluably by proper "psychological" factors: his bedside and consulting-room attitude. But this brand of psychology, which is to a limited extent part of the equipment of good salesmen, "ad" writers, lawyers, and confidence men as well as of doctors, touches but lightly the field of scientific psychology. The question, then, is whether scientific psychology has anything to contribute towards treatment of mental disorder and, if so, whether the practicing physician can and should be the one to apply such discoveries in dealing with individual cases.

The Committee differentiates clearly the "psychiatric attitude" and the "technical resources" of psychologist and psychiatrist. The former, it is felt, can be acquired readily by any intelligent doctor and will suffice for most cases of behavior abnormality. This, we believe, is a gross underestimation of what the psychiatric attitude can and should be. With experience, some (by no means all) doctors acquire the ability to handle behavior problems with considerable "insight" and success. It is no less than reasonable to expect, however, that a background of psychological training, thorough acquaintance with the field if not mastery of all the techniques, would enormously benefit even those endowed with an "intuitive" understanding

of human nature. The "technical resources" are considered less essential for the family doctor than the psychiatric attitude. "Obviously there are details of . . . methods of testing intelligence and so on which are reliable only when interpreted by specially trained individuals. . . . If he [the doctor] feels that his time-table allows patient and often prolonged conversations, he can begin to acquire technical resources." With this viewpoint the psychologist and psychiatrist cannot well agree. Few if any psychologists would maintain that it is possible to dispense with the services of a physician in dealing with mental cases; they realize that a discoverable organic difficulty may be at the root of the manifested behavior and, further, that the doctor's experience with sick people may have given him an invaluable tool in sympathetic understanding of the difficulty. But neither would the psychologist agree that it is possible to handle behavior problems adequately and intelligently without thorough psychological and psychiatric training. A standardized psychological test procedure, for instance, administered and interpreted by an expert, would seem to be as necessary a preliminary to subsequent procedure as is a thorough medical examination. The point is one more easily settled by opinion than by demonstration. At any rate, we may expect in the future a steady increase rather than any decrease in the body of specialized knowledge and techniques pertaining to the field of juvenile personality.

Whether or not the physician, the family doctor, of the future can afford to shoulder the burden of thorough psychological and psychiatric training, remains to be seen. The problem is closely related to the general one of specialization among medical practitioners. In this connection both doctors and psychologists might do well to bear three points in mind. First, the similarity between physical disease and behavior difficulties implied in the statement that "when . . . the individual child is in distress a well informed and alert physician is the obvious adviser," is, in the present state of science, largely a verbal one. Secondly, behavior troubles cannot be excised as can be a tumor nor can they often be treated successfully by a prescription. Diagnosis and cure of mental disorder are usually time-consuming processes. Thirdly, the average psychologist of to-day is rarely either interested in or fitted for the job. It is essential to recognize, also, that the organization and intra-group responsibility found among medical men makes it far safer to entrust decisions involving radical procedures to them rather than to psychologists who, at present, have no comparable safeguards to offer. The ultimate solution, we may expect, will come through gradual evolution rather than by sudden decision or decree, and will be strongly influenced by the direction of scientific progress as well as by social and economic trends.

Yale University HENRY W. NISSEN

Fields of Psychology. By GLENN DEVERE HIGGINSON. New York, Henry Holt & Co., 1931. Pp. xviii, 613.

Higginson's book is designed for three classes of students, those in whom a bird's-eye view may awaken particular interests, those "who do not plan to continue their psychological studies, but who, nevertheless, desire a fuller understanding of the subject matter than an elementary course can possibly provide," and those who find themselves in smaller departments where a diversified curriculum is not offered. Part I (72 pages) briefly discusses scientific method and

psychological systems. Part II (234 pages) considers evolution, animal psychology, the development of the human race, child psychology, and social psychology. Part III (182 pages) deals with educational psychology and applied psychology. Part IV (117 pages) is devoted to abnormal psychology.

The general point of view is functional and biological. The attempt to cover a very large number of topics has rendered the treatment in many places brief—a difficulty recognized by the author. Practically no space is given to physiological psychology, a chapter is given to comparative, to child, and to social, two chapters to educational, three to applied, and five to abnormal. This division seems markedly out of balance. Also a chapter on evolution and another devoted to archaeologic findings on primitive man seem a little out of place. Perhaps the chief contribution of the present work is that of sensing a need and assembling a wide variety of material under one binding.

University of Wisconsin KAI JENSEN

The Free-Association Method and the Measurement of Adult Intelligence. By HERBERT S. CONRAD and DANIEL HARRIS. University of California Publications in Psychology. Berkeley, University of California Press, 1931. Pp. 45.

To test the intelligence of adults who are averse to submitting to the process, the authors have sought to utilize the free-association technique and to construct from it a valid intelligence-test. The authors chose 120 words as stimuli, attempting to avoid ambiguity, slang, and polysyllables. The list contained 68 nouns, 29 adjectives, 22 verbs, and 1 preposition. The words were presented to 297 Jewish students, aged 11 to 15 yr., as a validating measure, since the results from this testing could be correlated with National Intelligence Test scores from the same subjects. The measure of validity which resulted was 0.78. Hence, it was supposed that the test would measure equally well or better the intelligence of adults.

The results from the free-association test were classified into three categories: type of relation between response and stimulus, such as the relationships of coördination, opposites, specifications, and inadequacy; vocabulary designation, such as hard-words, good-words, and misspellings; and reaction-time or speed, somehow (not clear to the reviewer) obtained from the number of omitted responses, revised omissions, and total omissions. These criteria were found to be very useful, while predication, definition, word-habit, repetition, slang, non-specification, multi-verbality, and exceptional and unclassifiable responses were discovered to be useless.

Apparently the reliability of each criterion, and its validity against the Army Alpha score, was measured on 15 adults, aged 18 to 22 yr. "Hard-word" responses proved to possess the highest reliability (0.87 on 166 children).

The paper contains 15 tables, 2 figures, and a bibliography of 18 references. The discussion ends with a somewhat deductive, non-experimental comparison of the test with Stanford-Binet and other standard tests. Among its multifarious virtues are its novelty, acceptability to adults, appropriateness for group (and even mail) administration, sufficient disguise to ward off cheating when taken at home, and its proved validity for group comparison.

University of Oregon R. D. ADAMS

Sociologues d'hier et d'aujourd'hui. By GEORGES DAVY. Paris, Félix Alcan, 1931. Pp. 308.

It is Davy's contention that an exact knowledge of contemporary French sociology is impossible unless we are acquainted with its sources. From this point of view, the title of the book is somewhat misleading. It is not a review of past and present sociologists but is rather an analysis of certain recent authors whom Davy feels are largely responsible for present sociological trends in France.

The volume is divided into four parts, each of which is almost entirely devoted to the work of a single author. Part I is a survey of the works of Espinas. Part II considers a single aspect of Durkheim's sociology which best illustrates that author's method; namely, his theory of the origin of the family. Part III is a review of the method of McDougall, and Part IV is an integration of the writings of Lévy-Bruhl.

Each part of the book gives an excellent digest of its particular topic. The exposition is, however, almost entirely uncritical. Davy has made very little attempt to show the relationship of one author to another or to trace the modifications of older doctrines as they appear in more recent writings. This is due, perhaps, to the fact that the book is a series of separate studies. The scope of the volume is extremely nationalistic. Even if we grant that France was the cradle of sociology, it seems unfortunate that McDougall and Spencer should be the only non-French authors considered. The names of Gumplowicz, Ward, Giddings, Small, Sumner, and Pareto as well as those of more recent sociologists are entirely omitted.

Harvard University HADLEY CANTRIL

Prediction Factors in Probation: A Study of 1515 Probation Cases of Ramsey County, Minnesota, for the Years 1923-1925 Inclusive. By ELIO D. MONACHESI. Minneapolis, The Sociological Press, 1932. Pp. ix, 117.

The author, using the court records of individuals' histories before and after being put on probation, found some facts such as age, easily classified; whole other facts such as social status required conceptual treatment. The reliability of the latter classifications was tested by reclassification after a number of months, and 60–80% of the individuals were classified the same.

The violators of probation were placed in one group and the non-violators in another, and tables were constructed to show what facts were most characteristic of each group. Of the children of foreign born parents 73% were non-violators, and of native born parents, 68%. Children of good economic status, good and unbroken homes, and those with parents of the same religion, also showed a large percentage of non-violators. Children who leave home at an early age and those who have had home difficulties, are numerous among the violators of probation. The number of non-violators is directly related to the amount of education previously received. Space does not admit of giving the results of the comparison of numerous other factors. The author concludes that although court and probation records are not complete or exact and although no one type of fact is a sufficient basis for predicting the outcome of probation, yet combinations of a number of facts make possible fairly accurate predictions as to which persons will be likely to violate probation.

Tables constructed on the basis of such facts as these may be used either to select those to be refused probation or to decide upon the amount of supervision the individual probationer will need. The author, using the methods employed by Burgess, Glueck, and Vold in the study of paroled criminals, has begun the construction of prediction tables which can undoubtedly be made to serve as guides in dealing with candidates for probation. He has also made a contribution to the scientific study of predisposing tendencies toward conduct, and to a future scientific ethics.

A good bibliography and an index are appended.

Leominster, Mass. EDWIN A. KIRKPATRICK

Tonpsychologie und Musikästhetik: Art und Grenzen ihrer wissenschaftlichen Begriffsbildung. By HEINRICH SCHOLE. Göttingen, Vandenhoeck & Ruprecht, 1930. Pp. 139.

This work is not intended to be a systematic presentation of tone-psychology and musical aesthetics. The author's purpose is rather, as the subtitle indicates, to consider the types of concepts which are valuable in these fields and to show their limitations. Schole lays stress upon two methodological principles. In the first place, a careful "phenomenological" examination of the elementary phenomena of music is needed. The concept of atomistic elements must be abandoned, the criticisms by speculative *Geisteswissenschaftler* of experimental psychology thus losing much of their force. Secondly, psychological research must devote itself more than hitherto to complex musical forms.

Schole's treatment of musical elements, in the first part of the book, is limited to their psychological or experiential aspect. He disregards neural factors completely and refers to stimuli only for the sake of convenience. Partly under the influence of Köhler's studies of simple tones, Schole has been led to a peculiar doctrine of tonal attributes. A simple tone possesses but one single attribute, *Tonkörper.* Pitch, intensity, *Tonfarbe,* brightness, vocality, and volume must be considered results of relational activity "die aus der vergleichenden Betrachtung von Qualitätsmannigfaltigkeiten gewonnen werden, aber der einfachen und erlebnismässig unzerlegbaren Qualität kein neues Merkmal hinzufügen können" (p. 13). Among these six concepts, intensity and *Tonfarbe* are found in the *Tonkörper* itself, while the other predicables, in so far as they are not synonyms for the first two, owe their existence to still more complicated relational functions leading beyond the simple quality itself. Schole's insistence that true attributes must be found immediately within the tone is quite foreign to the more generally accepted view that tonal attributes are conceptual products resulting from comparison of tones under particular instructions.

Much of the remaining material in the first part of the book is critical rather than constructive. The author attacks mercilessly Ernst Kurth's approach to the problems of the psychology of music.

The second part consists primarily in an analysis of the first movement of a Beethoven sonata (*Op. 31*) and concludes with a summary of the author's theory of the nature of musical hearing. This portion of the book will be of greater interest to musicians and aestheticians than to psychologists.

Students of the psychology of music will find throughout the book many stimu-

lating suggestions, even though the writer's arguments are not always convincing. American readers will note with surprise that Schole has cited only writings in the German language.

University of Rochester C. W. Fox

Intelligence and Disease. By SHEPHERD DAWSON, assisted by J. C. M. CONN. Special Report Series, No. 162, issued by the Medical Research Council. London, His Majesty's Stationery Office, 1931. Pp. 53.

The work of this study was begun in June, 1922, by Dr. H. J. Watt, who died in 1925. The investigation is complementary to a research conducted by the late Professor D. Noël Paton, Dr. Leonard Findlay, and their collaborators, the results of which were published by the Medical Research Council in 1926 under the title *Poverty, Nutrition, and Growth,* forming part of a scheme for the study of child life initiated by the Council. In that study it was found on some 14,000 children living in the slum areas of Glasgow, Edinburgh, Dundee, in a rural mining area, and in an agricultural area that of the various external factors studied "maternal efficiency, as indicated by the care of the child and of the home, was the only one which played a significant part in determining nutrition in the child. Over-crowding, estimated either by the size of the family or by the amount of air-space available per person, and poverty, as indicated by income per person, did not seem to influence to any significant degree the child's nutrition" (5 f.). Whether this maternal efficiency is innate intelligence or a result of environmental conditions under which the mother lives was a question that needed further study. The present investigation was continued in Dr. Findlay's department at the Royal Hospital for Sick Children and the Ruchill Hospital, Glasgow, with a view to throwing light on that problem. The mental testing since 1925 was done by J. C. M. Conn, who had formerly worked with Dr. Watt.

The testing involved over 1020 patients, ranging in ages from 3 to 14 yrs. The 3-yr.-olds were evidently superior samplings (p. 11), and the 56 above 11 yr. were probably inferior. The mean age of the entire group was 7.7 yr. Burt's translation of the 1908 Binet scale was used. The correlation between age and I.Q. was found to be approximately zero. The mean I.Q. of the different age groups ranged from 0.82 to 0.96 (highest and lowest ages, respectively), but usually was near 0.90. The children seem to have been fair samplings of the normal children of the same area, so far as intelligence is concerned, when not affected by certain specific brain diseases. The problem, then, centered on the question: "Does illness in general retard intellectual development or is it only certain diseases that have this effect, and what are they?" (P. 21.) Every patient was tested before, during, and immediately after his illness; and a retesting was given during convalescence and also when the patient had recovered. In addition, patients suffering from any particular disease were compared with their healthy siblings, who were brought to and tested in the hospital with the same tests as those used in the case of the patients. Full data are presented in tables, and results are carefully considered as to reliability and to the effects of spurious factors.

It is concluded that "disease on the whole does not appear to have any appreciable effect on intelligence" (p. 51), even though diseases of the spinal cord be included, as well as neuritis and chorea. On the other hand brain diseases—epilepsy, post-

encephalitis lethargica—and certain ductless gland diseases were found to be associated with mental deterioration. No such clear evidence of deterioration was found generally in the relatively few cases of the Parkinsonian syndrome, spastic diaplegia, and cerebral and cerebellar tumors. While "chronic illness did not appear to retard seriously the development of intelligence," acute stages of illness was on the whole associated with a "slight improvement in mental efficiency" (p. 51). This improvement seemed to be due to a slight hypersensitivity in the acute stage. Unfortunately the number of cases studied was small in the case of several of these specific brain diseases.

There is a bibliography of 56 titles. J. P.

Handbook of Psychological Literature. By C. M. LOUTTIT. Bloomington, Ind., Principia Press, 1932. Pp. viii, 273.

Designed by its author for those psychologists who find themselves at a loss while searching in unfamiliar fields, this volume may be of interest to others, if only to remind them of the bulk of published material that has accumulated in psychology and its allied disciplines. After early chapters devoted to bibliographic techniques and to the various sources of reference for the student of psychology, the author presents as his chief contribution a list of 1084 periodicals which contain varying proportions of material of interest to researchers in the several phases of psychology. He warns us early that "the number of papers of psychological value may be conservatively estimated as over 100,000" (p. 4). Later, in a more expansive mood he ventures the statement that "conservative and very approximate estimates . . . indicate something over 150,000 as the number of contributions to the literature" (p. 79); and as one reads over this appalling catalogue of journals, living and dead, one is not tempted to deny the author the added 50% of his second estimate.

The list which, the author tells us, "has been selected almost entirely from the *Union List of Serials* and its *Supplement*" (p. 137) arranges alphabetically periodicals which contain matters of psychological interest. In most cases the name of the publishing organization or place of publication, together with the date of first publication and an indication of its later fate, is given; but this procedure is not followed rigidly. Here are to be found the names of journals of all sorts, from the most staid of the *Zeitschriften* down to long defunct periodicals of phrenology, hypnotism and animal magnetism. Since numerous journals in foreign tongues are included, one might almost write a history of psychology by scrutinizing the list in detail and noting the few hardy survivors among the large numbers of decadent, decayed and fossilized remains interred in these columns.

The author points out that such a catalogue can have final value only after careful revision, and he writes that "every effort has been made to ensure accuracy, but if errors have occurred (*sic*) the author would appreciate their being called to his attention" (p. 137). The space-limits of this review will not admit of detailed criticism, but the following are offered as typical of changes which should be made:

(1) Some consistent rule should be applied to the use of upper and lower case letters. Thus *Psychological Index* appears with both initial letters capitalized on one page (29) while only the first is so treated on the next page. Such vagaries of capitalization occur frequently, and could be eliminated by careful editing. The

present reviewer is unable to see any gain in the use of lower case letters for all but the initial word in the titles of the journals themselves; it would seem advantageous to follow the more customary procedure in this regard.

(2) The names of the journals appearing in the earlier chapters should be carefully checked and corrected. It is astonishing that the author should have failed to notice three references to a French contemporary as "L'Annee Psychologie" (pp. 94, 101, 102) and one to what the author calls "Wundt's Philosophisches Studien" (p. 23). At least two other journals not available to the reviewer appear to have suffered distortions in title which are contrary to fundamental rules of the German language. In the section on handbooks, there is no reason to abbreviate the title of Abderhalden's *Handbuch der biologischen Arbeitsmethoden* in the curious fashion employed by Louttit.

(3) Newer sources which were available two or more years before the publication of this volume should be included. Thus Dumas' *Nouveau Traité de Psychologie* (1929) should replace the earlier edition cited. The general bibliographic citations from various issues of *Science* (p. 10) might well give way to a single reference to the article in the 1929 *Psychological Bulletin* which presents the standard form of reference adopted by the conference of editors of psychological journals. Some reference should be made to Bethe's *Handbuch* in the appropriate section; and Eisler seems at least as deserving as Baldwin of a place in the encyclopedic section, instead of being relegated to a corner among the dictionaries.

The volume closes with appendices which contain such items as an index of subjects and names, a categorical index of journals, and a list of references documenting the volume, as well as lists of special collections which may be of value to psychologists. Several tables throughout the volume are provided to show distribution of journals by countries and by years. If the volume meets with sufficient demand to justify revision, the author would gain by submitting his manuscript to psychologists here and abroad in an effort to make it as representative and as accurate as possible.

Cornell University JOHN G. JENKINS

660 Runaway Boys. Why Boys Desert Their Homes. By CLAIRETTE P. ARMSTRONG. Boston, Richard G. Badger, 1932. Pp. 208.

In this monumental statistical study, 660 runaway school boys, whose entire records, covering physical, psychological, psychiatric, neurological and serological examinations, were on file in the clinic, were compared with similar records of two control groups who had never run away from home—one group charged with incorrigibility at home and the other with unlawful entry. All three delinquent groups were compared in various measurements and ratings with unselected school children. All the runaways in this study were "delinquents." Runaways held in court as "neglected children" or on an "improper guardianship" complaint were excluded. Scientifically objective almost to the point of dullness in earlier chapters, this book rises to heights of oratory in the summary and conclusions. The author, having shown that many criminal careers start with running away from home, thinks that society should shoulder the responsibility and do something to prevent it.

Moralists of an earlier age might have reasoned so far—in fact they evidently intended to stop runaways when they made it legally a delinquency for the child

to desert his home without just cause. The author, however, has discovered that remaining in the home may be to many children a severer hardship than any penalty the law can impose on them for leaving it. The common assumption that boys leave home chiefly from *Wanderlust* proved erroneous. Of the 660 boys studied, 590 never left New York. The basic motive for nearly every runaway was escape. In 12% of cases, the boys had been severely beaten. Another 32% feared punishment. When no physical fear was involved, there was often fear of humiliation. The public school with its rigid requirements and lack of provision for the dull nonverbal type of boy is held by this author to be a major cause of truancy, so often the stepping-stone to other delinquencies." "A problem of first importance, practical and lending itself to immediate solution, confronts the schools as they stand today. It is not primarily how to cure nervous, maladjusted children but how to avoid making them."

West Springfield, Mass. MABEL F. MARTIN

Untersuchung der Entwicklung des Zeichnens. By HERBERT GRAEWE. Halle (Saale), Hermann Schroedel, 1932. Pp. vi, 180.

In this study, Græwe has attempted to trace the course of development in children's drawings from a broader, less specialized point of view than many of his predecessors have taken. Instead of obtaining a small number of drawings from each of a large number of subjects, he secured from each of his 143 subjects drawings of many different types made under a variety of conditions. In all, 6250 drawings were obtained. The subjects included a group of 37 kindergarten children ranging in age from 3 yr., 2 mo. to 6 yr., 6 mo.; 96 grade school children between the ages of 6 and 14 yr.; and 10 adults. The drawings tests for the kindergarten children included copying geometrical figures and pictures of common objects, drawing simple objects from a model placed before them, drawing the same objects from memory after a brief interval, drawing a picture of their own mother, a man, a child and a table from memory, and drawing a witch from imagination. The tasks set for the older children were of a similiar nature but slightly more difficult.

Those who have even a slight acquaintance with previous work in the study of children's drawings will not be surprized to find that specifying that the children were to draw "their own" mother had no effect upon the schematic type of drawing produced by these young children. Drawing the witch proved to make no more demand upon "imagination" than the other types of drawing, since the children merely attempted to reproduce the conventional Hallowe'en figure.

No statistical analysis of the results is given apart from a few percentages, but there is a general descriptive treatment of the changes that occur with age in each type of drawing. There is little or no attempt at correlational treatment even in terms of casual observation, hence whatever advantage might theoretically accrue from the intensive study of a small number of cases is lost. Each type of drawing and each general aspect of drawing (as motor coördination, representation of space, etc.) is considered separately.

A bibliography of 263 titles and reproductions of 26 drawings are appended. There is no index.

University of Minnesota FLORENCE L. GOODENOUGH

Mutter und Sohn. By KARL CLAUSS. Pädagogisches Magazin. Langensalza, Hermann Beyer & Sons, 1931. Pp. 104.

As a result of the World War many children were educated without the guidance of a father. The author made an intensive study of 189 boys in this class of ages ranging from 8 to 20 yr. Information was obtained from the subject's mother, teacher, and companions, by means of interviews, questionnaires, and casual conversations. The results were treated statistically. Special attention was given to the occupational status of the family and the psychological type of both mother and child, the author adopting G. Kerschensteiner's simple division of personalities into active and contemplative types, corresponding broadly to the well known classification of extraverts and introverts. In general, boys of both types suffer an educational handicap from purely maternal direction. This is greatest when mother and child are of opposite psychological type, but boys of contemplative type suffer most. Exaggerated attitudes, one-sidedness and failure to develop a sense of reality, cover most of the defects encountered. Where a vivid and pleasing memory of the father persists (*Vaterideal*), as is often the case in children from the military and official classes, no bad effects occur. It would be interesting to make similar studies among boys in other countries affected by the war, and the method might well be extended to groups of children of either sex, deprived during developmental years of either parent, whether by war, accident, divorce or other cause of separation.

University of Cincinnati CHARLES M. DISERENS

The Psychology of Exceptional Children. By NORMA V. SCHEIDEMANN. Boston, Houghton Mifflin Co., 1931. Pp. xi, 520.

This treatise is comprehensive in the topics with which it deals and in the treatment of each. The style is lucid, to the point, and very readable. The comprehensiveness of the text can be indicated adequately by a list of the subjects discussed: origin of trait differences, the concept of general intelligence and its measurement, speech disorders, handedness, the subnormal child, special types of feeble-mindedness, gifted children, the psychoneurotic child, the psychopathic child, the deaf child, the blind child, the word-blind child, the delinquent child and other types of exceptional children, such as the epileptic, the hysterical, and the undernourished.

There is a marked difference, however, in the quality of the various chapters. The handling of the material on the psychoneurotic, the psychopathic, and the congenitally word-blind child fall far below the treatment of speech disorders, handedness, and feeble-mindedness. One has a feeling that the author was not always sure of herself, and through uncertainty, did not treat some subjects with the same insight as she did others. The citations throughout the book are good. Illustrative cases are used throughout.

Western Reserve University L. DEWEY ANDERSON

Experimental Child Study. By FLORENCE L. GOODENOUGH and JOHN E. ANDERSON. New York, Century Co., 1931. Pp. xii, 546.

This book is "frankly experimental and observational in tone rather than philosophical." As such it is a distinct contribution to the field of child psychology.

The authors have followed a somewhat novel plan of presentation in organizing the book into forty-seven relatively short chapters. Most of these chapters have brief preliminary discussions of the problem under consideration, followed by one or more suggested experiments or test exercises for students to perform. Thus, the book in many ways is an elaborated laboratory manual which has been so arranged that it can be used as the basis for a course outline for class discussions.

Two of the very best features of the book are its bibliography and glossary. The bibliography of 403 titles is rather carefully selected, and has the added feature of being based largely on publications in English. This is a real contribution to students, both from the standpoint of their own reading and from the standpoint of the availability of library materials. Then, the glossary is of practical assistance to students who are incompletely oriented in psychological terminology.

The book furnishes an excellent reference for classes, but its main limitation is probably that it is better adapted to extra work than to regular class work. It appears to assume a foundation in psychology more extensive than is possessed by the average student just beginning child psychology. At the same time, its policy of treating subjects extensively rather than intensively does not seem to appeal to graduate students with special interests in the field of child psychology. Many of the experiments are not adapted to graduate work. A second matter which should be considered is that the book is pointed avowedly towards the pre-school or early-school child. This limited scope or perspective circumscribes its usefulness in classes where the instructor must present child psychology "from the cradle to the grave." Withal, the book could be used profitably as a second text in many courses.

George Peabody College for Teachers PAUL L. BOYNTON

Die Psychologie des Grundschulkindes in ihrer Beziehung zur kindlichen Gesamtentwickelung. By OSWALD KROH. 10th ed. Pädagogisches Magazin, Heft 1122. Langensalza, Hermann Beyer & Sons, 1931. Pp. 353.

This book emphasizes the child's personality. The main phases of development are reviewed and then the child of the primary grades is more thoroughly discussed.

The ego, the basis for intellectual development; the perceptual, learning and memory processes; the intellectual mechanisms; the development of emotions and values, of moral, religious and esthetic experience; the development of social attitudes, of expression, of interests in play and work, are treated in a stimulating fashion. The four years of *Grundschule* are treated as corresponding to a definite stage in child development. The author discusses the functions of the *Grundschule* from the standpoint of his findings.

The book cannot claim to be a complete treatment of the subject. Developments abroad are completely ignored; the words "test" or "intelligence" hardly occur, the psychology of learning and adjustment are treated rather sketchily. Yet the viewpoints are stimulating, the use made of the findings of experimental psychology is interesting and the general emphasis on personality development and on mental content and functioning as aspects of this development, is very valuable. The

author is not at all concerned with the "efficiency" of teaching in a narrow sense. To him the teacher's fundamental responsibility is that of guiding the child's harmonious development.

Wayne County Training School, Northville, Michigan THORLEIF G. HEGGE

The Measurement of the Intelligence of Young Children by an Object-Fitting Test. By RUTH ELLEN ATKINS. Monog. Series No. V, The Institute of Child Welfare. Minneapolis, Univ. Minn. Press, 1931. Pp. xii, 89.

Like practically all the studies in child psychology which come from the Institute of Child Welfare, this study is a real contribution to the literature of the field. It has evidently been planned thoughtfully and conducted carefully. The cogency of the interpretive analyses deserves particular mention.

The test is probably more self-motivating for young children than some examinations which have been designed to cover the same age ranges of one and one-half to five and one-half years. It is based on such commonplace objects as spools, cups, salt shakers, tops, and pencils, with all of which most children in these age ranges like to work and play. The test is short; it is available in two forms; its reliability coefficient is around 0.87; it is strictly a non-language examination; the situations involved are sufficiently controlled so as not to confuse the subject by their complexity; and estimates of its validity show that it has a practical clinical significance. The last chapter in the monograph is a manual of directions for giving and scoring the test and for translating its scores into mental ages.

George Peabody College for Teachers PAUL L. BOYNTON

The Wild Boy of Aveyron. By JEAN-MARC-GASPARD ITARD. Tr. by George and Muriel Humphrey. Introduction by George Humphrey. New York, The Century Co., 1932. Pp. xxiv, 104.

In 1799 there appeared in the *Journal des Débats* a letter by a certain Bonaterre describing a wild boy (probably about twelve years of age) who had been taken in the woods of the Department of Aveyron. The boy "had been found almost unclad, wandering about the outskirts of the forest in which he had apparently lived for some years" (p. vi). "Other indications of an entirely isolated, precarious and wandering life," writes Itard, "are the nature and the number of scars with which the child's body is covered" (p. 9). The boy had been seen over five years earlier entirely naked and fleeing at the approach of man. He was taken to Paris where inquisitive people "looked forward with delight to witnessing the boy's astonishment at the sights of the capital." Some people otherwise "commendable for their insight . . . believed that the education of this child would only be a question of some months, and that he would soon be able to give the most interesting information about his past life. In place of all this," says Itard, "we see a disgustingly dirty child affected with spasmodic movements and often convulsions, who swayed back and forth ceaselessly like certain animals in the menagerie, who bit and scratched those who opposed him." Though Pinel examined the boy and reported him an incurable idiot, instead of merely a wild boy, Itard, who had early

attained some distinction in medicine and at twenty-five years of age had been appointed physician to a new institution for deaf mutes, was willing to try his view of the effect of training on the boy.

Pinel's report is unfortunately not available today. Of the two memoirs translated in this book, the first one, "First Developments of the Young Savage of Aveyron," was originally published in 1801, and the second, "A Report Made to His Excellency the Minister of the Interior," which is divided into three series on the development (a) of the "functions of the senses," (b) of the "intellectual functions," and (c) of "emotional factors," was submitted to the minister by request in 1806. The translation is made from the reprinted edition of 1894, *Rapports et Mémoires sur le Sauvage de l'Aveyron,* now out of print. Itard devoted five years to the training of the boy who died in 1828 at approximately forty years of age. Itard lived ten years beyond this time.

The story of the aims and methods used in the laborious attempts at the education of this boy, results obtained, and Itard's own views, hopes, and discouragements, are interestingly and clearly narrated. The translators and publishers are to be complimented on the excellency of the translation and for making available these memoirs to English readers.

J. P.

The First Two Years. By MARY M. SHIRLEY. Monog. Series No. VI, Vol. I, The Institute of Child Welfare. Minneapolis, Univ. Minn. Press, 1931. Pp. xv, 227.

This is distinctly an intensive research. It is a report of an analysis of 16 babies from birth to two years of age. Though the study is primarily concerned with locomotor development, records of each baby were kept throughout the entire period on (1) anthropometric measurements, (2) health, nutrition and general well-being, (3) motor coördination, (4) sensory development, (5) speech development, (6) interests, and (7) behavior and personality traits. The babies involved were from an admittedly "upper class" group of homes, but since the author is not attempting to generalize for all babies, this selectivity of cases does not vitiate the study. The investigation was made under as carefully controlled conditions as one could expect in a field where private homes have to be laboratories and where the infants in these homes have to serve as subjects. Child psychology needs more of such analytical studies.

George Peabody College for Teachers PAUL L. BOYNTON

Neuropsyche und Hirnrinde: I. Der bedingte Reflex und seine Bedeutung in der Biologie, Medizin, Psychologie und Pädagogik; II. Physiologische Grundlagen der Tiefenpsychologie unter besonderer Berücksichtigung der Psychoanalyse. By N. E. ISCHLONDSKY. Berlin and Wien, Urban & Schwarzenberg, 1930. Pp. xviii, 328; xv, 356.

In two laborious volumes the author expounds Pavlov's theory of the conditioned reflex and applies it to the fields of psychology and medicine. The first volume recapitulates the contents of Pavlov's two books concerning conditioned reflexes. The method and the theory derived from its use are described at length with the aid of

numerous colored diagrams which sug[illegible] to the teacher their suitability for blackboard illustration. The few pages of the first volume (71-75), however, in which the author describes his experiments on tactile localization in skin areas grafted from forearm to face and explains the "education" in localization of the grafts is exciting reading. For the rest, the reader can gain nothing from a perusal of the first volume which cannot be better acquired by consulting Pavlov's writings. The author's exposition is devoutly orthodox and hence uninteresting.

The second volume is a *tour de force* of mental gymnastics in the explanation of complex mental processes and their aberrations in terms of conditioned reflex theory.

Cornell University H. S. LIDDELL

Our Movie Made Children. By HENRY JAMES FORMAN. New York, The Macmillan Co., 1933. Pp. viii, 288.

Mr. Forman has summarized, in popular form, the findings of twelve separate investigations on the influence of motion pictures on children. These investigations were carried out under the auspices of the Motion Picture Research Council and supported by the Payne fund. The titles and authors of the studies are listed with no reference to their place of publication or availability.

On the average 28,259,000 minors, of whom over eleven million are under fourteen years, attend the movies every week in the United States. The average movie attendance per child is one movie a week. These children, even the young ones, remember almost as much as do adults of what they saw in a picture. The pictures are powerful instruments of propaganda, *e.g.* a single picture was shown clearly to establish or to remove, race prejudices. The children are deeply stirred emotionally by even mildly exciting movies; this is shown by psychogalvanic reactions and pulse rate, and by the fact that their sleep is disturbed for several nights after seeing a motion picture.

The section of life displayed by the movies is a distorted one, with sex and crime themes and luxuried leisure class predominating. Since the children were found to accept the pictures as true representations, and to grasp only individual scenes rather than the story as a whole, the pictures appear to have encouraged many children to delinquency and crimes, both by having stimulated desires for luxury and leisure, and by giving instruction in methods of 'petting' and techniques in committing crimes.

Psychologists will probably prefer to read the reports of the original investigators. The present volume, as a popular interpretation which emphasizes the need of social and educational control of children's movies, is impressive. At times, however, it labors a point by unnecessary repetition.

University of California NANCY BAYLEY

Developing Attitudes in Children. Proceedings of the Mid-West Conference of the Chicago Association for Child Study and Parent Education, March, 1932. Pp. vii, 156.

This volume consists of ten papers presented at the Mid-West Conference on Developing Attitudes in Children. These papers are varied in content and differ

in their general approach. Some of them are philosophical in nature. This is true of the papers by Harry Elmer Barnes, Glenn Frank, and Harry Gideonse. There is a persistent warning throughout some of the papers relative to racial bigotry, international relations, and citizenship training, lest we carry over into the teachings of children the prejudices and biased attitudes of our present time. The papers vary from the strictly scientific approach made by Ruth Peterson, "The Effect of Motion Pictures on the Social Attitudes of Children," to the rather colorful philosophical approach already suggested. There is, however, a general unity in purpose and coherence in basic principles and foundations upon which the papers are built.

In the first paper Hartshorne emphasizes the relation between ethical self-control and desirable attitudes. Throughout the various papers the importance of the early childhood years for training and attitude-development is stressed. The complexity of modern life, with its many materialistic developments in a complicated social and political order, makes it most essential that growing boys and girls do not develop undesirable, twisted and warped attitudes. This point is stressed in many of the papers. Parents, teachers, and others concerned with child training will find these warnings and varied suggestions helpful.

North Carolina State College K. C. GARRISON

Die juvenile amaurotische Idiotie. Klinische und erblichkeitsmedizinische Untersuchungen. By TORSTEN SJÖGREN. Lund, Berlingska Boktryckeriet, 1931. Pp. 197-425.

This monograph deals with a careful study of approximately one hundred cases of amaurotic idiocy in the institutions for the blind of Sweden. Characteristic changes in structure of the retina, progressive neurological symptoms (posture, gait, and speech), as well as more psychological patterns of behavior (dementia, negativism, and compulsions) are recorded in detail for each case from the onset to the termination of the disease in death. A differential diagnosis of the symptoms of amaurotic idiocy from other juvenile diseases (paresis, juvenile diffuse cerebral sclerosis) is made.

From a statistical study of the family histories the author has found no pathological evidence in the heredity differing from the normal, but he did find greater percentage of blood relationship among the parents of amaurotic idiots than among normal individuals. He concludes that amaurotic idiocy is a recessive monohybrid hereditary character.

A statistical report of the rate of appearance of the disease in Sweden during the last fifty years as well as its prevalence in different geographical areas is also included.

Sarah Lawrence College THEODORA M. ABEL

The Development of Learning in Young Children. By LOVISA C. WAGONER. McGraw-Hill, New York, 1933. Pp. xiv, 322.

This is a practical and very useful book on child psychology, mainly restricted to the first five years of life. The author stresses the knowledge of what may be expected of a child. She warns against the frequent interpretation of normal fea-

tures of behavior as indicating "a problem child." The first five chapters cover the general ground of learning in animals and the human species and end with "maturation versus learning." Control of the body in walking, manipulating, dressing, etc., is then described. Social relations and the elimination of undesirable actions are then made clear. Sensible advice is given on sex education beyond the early years of life. Speech development, learning to eat, to sleep, to eliminate from the bowels are well discussed. The last chapters make it very clear that adults cannot deal with children so as to help them to grow up, if they regard their duties with the child as a mere avocation or, worse, as a mere nuisance. The book is a good child psychology for junior college students or young mothers.

University of Missouri MAX F. MEYER

Jugendpsychologie der Gegenwart. By OTTO TUMLIRZ. 2nd ed., enlarged. Berlin, Junker & Dunnhaupt, 1933. Pp. 97.

This is a valuable review of the contemporary psychology of childhood and adolescence. There are numerous references, mostly to German literature. The following quotations indicate the point of view taken.

"The tendency now prevails to disregard the observation and collection of single traits. We do not review mental development in longitudinal cuts, studying the development of single abilities from early childhood to adulthood. On the contrary, we make horizontal cuts and sketch broadly the various *levels of development.* These levels are related to personality, to a whole which expresses itself in definite main characteristics." "The example of Meumann, the influence of educational interests on psychological research, the preference for a few special problems, as for example that of intelligence and intelligence testing or that of vocational aptitude and its measurement, have resulted in innumerable specialized investigations while at the same time the child himself, until quite recently, remained relatively unknown." Earlier tendencies are criticized.

University of Michigan THORLEIF G. HEGGE

The Dynamics of Therapy in a Controlled Relationship. By JESSIE TAFT. New York, The Macmillan Co., 1933. Pp. ix, 296.

This is largely narrative description of 47 hours the author spent with two 7-yr.-old "problem" children. The section dealing with Helen appeared in the *Psychoanalytic Review* of October, 1932. The 31 "treatment contacts" with John were intended as preparation for placement in a foster home. Psychological data are conspicuous by their absence. The philosophy and methodology of the author are admixtures of those of the psychoanalyst, the social worker, and the free-activity advocate in education.

University of Cincinnati MILTON B. JENSEN

Case Studies of Normal Adolescent Girls. By ELSIE M. SMITHIES. New York, D. Appleton & Co., 1933. Pp. iii, 284.

This is a book of case studies of high school girls who have had scholastic difficulties because of personality maladjustment. Each case is described in detail with regard to symptoms, environmental conditions, health, mental tests, treatment,

and degree of satisfactory recovery. It is an excellent book for parents and teachers interested in typical problems of the adolescent and is full of wholesome remedial suggestions.

Wesleyan College, Macon, Ga. RALEIGH M. DRAKE

The Philosophy of Physical Realism. By ROY WOOD SELLARS. New York, The Macmillan Co., 1932. Pp. 481.

This book is a treatise on epistemology in which the author develops his theory of critical physical realism. There is little in the book of interest to the psychologist.

West Virginia University JOHN E. WINTER

Scepticism and Construction. By CHARLES A. CAMPBELL. New York, The Macmillan Co., 1931. Pp. xxiv, 322.

This essay is an attempt to revive Bradley's sceptical epistemology and extend it to support the phenomenological concept of reality or truth.

Hobart College FORREST L. DIMMICK

Common Sense and the Child. By ETHEL MANNIN. Philadelphia, J. B. Lippincott & Co., 1932. Pp. 314.

This is a plea for leaving the child alone. For parents to keep the child clean and tidy and to make it "behave nicely," is only to "do them credit" at the child's expense in freedom, initiative, and happiness. On the other hand allowing the child freedom, naturalness, and lack of restraint, except for physical dangers, results in normal sex life and emotions, greater honesty and directness in the activities of life, and greater happiness. The prescription is probably as good for the majority of American as for English children. The book is gauged for the average mother and for elementary school teachers. J. P.

Klinische Untersuchungen. By FRANZ KOELSCH. Handbuch der biologischen Arbeitsmethoden. Abt. IV, Angewandte chemische u. physikalische Methoden, Teil 16, Heft 2. Berlin, Urban & Schwarzenberg, 1932. Pp. 289-496.

A report of the clinical methods of determining occupational diseases due to physical, chemical, and biological defects. The use of the ergograph and dynamometer is indicated but no record of results is made. The article is of interest from the strictly medical point of view.

Sarah Lawrence College THEODORA M. ABEL

Peace of Mind and Body. By W. S. WALSH. New York, E. P. Dutton & Co., 1933. Pp. 249.

This book, written for the general reader, is concerned with the effects of suggestion upon mental and physical health. Such topics as distraction, fatigue, the influence of drugs, temperament, psychoanalysis, etc., are treated in simple language without marked loss of accuracy. The book may prove helpful to those who strive to follow its advice.

University of Vermont P. H. EWERT

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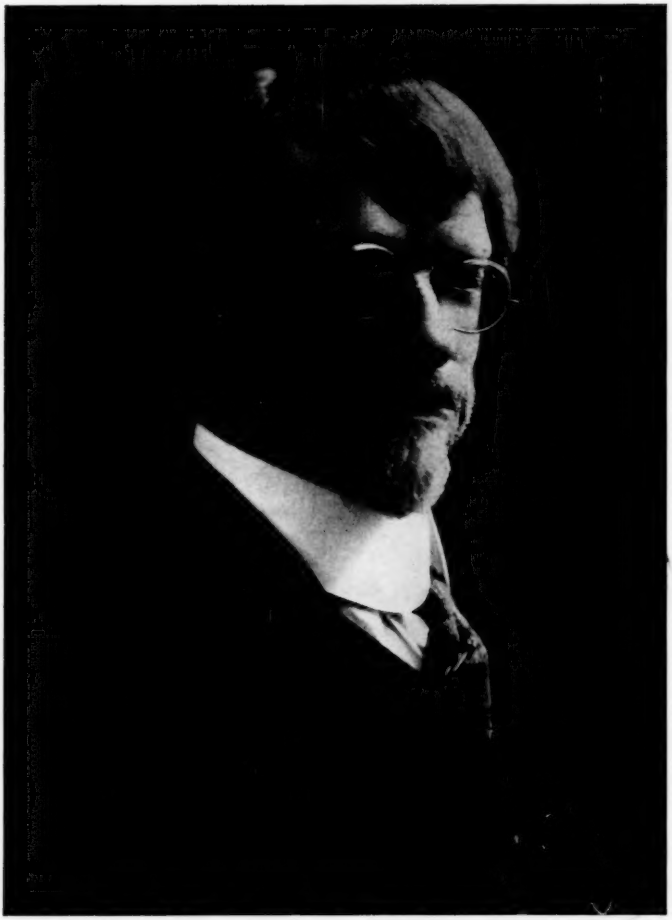

Howard C Warren

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